Proceedings of the Seventh Symposium on Containment of Underground Nuclear Explosions—Volume 1

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Clifford W. Olsen, Scientific Editor

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Preface and Acknowledgments

This is Volume 1 of two unclassified volumes of the Proceedings of the Seventh Symposium on Containment of Underground Nuclear Explosions. The Symposium, held September 13–17, 1993, at the Boeing Space Center East in Kent, Washington, was a meeting of workers at all levels in the science and technology of containment. Papers on containment and related geological, geophysical, engineering, chemical, and computational topics were included. The Seventh Symposium was quite possibly the last of the containment symposia, and we sincerely hope that these proceedings and those from earlier meetings will preserve some of the vast experience and expertise that was a part of the containment community.

We express our sincere thanks to Boeing for hosting the Symposium, and we give special thanks to Dr. Hal Ahlstrom and Ms. Debbie Petersen for their splendid cooperation in providing such fine facilities. Special thanks are due to S-Cubed and Raytheon Services Nevada for underwriting the Tuesday evening reception. Finally, we give our sincere thanks to Karen Roland and Marie Kaye for their invaluable support.

The Program Committee wishes to thank all presenters and attendees for their support, and gives special thanks to the session chairpersons: Ray Cornell, Hal Goldwire, Mel Hatch, Barbara Harris-West, Roger Jacobson, Jim Kamm, Willy Moss, Bill Proffer, Rachel Sandmann, Bob Swift, and Rick Warren.

We would also like to remember John Kalinowski of EG&G, who lost a battle with cancer in July 1993. John was not widely known, though he chaired a session at the Fifth Symposium, since most of his many years of work on containment were behind the scenes. His expertise on diagnostics and his unfailing good humor will be missed.

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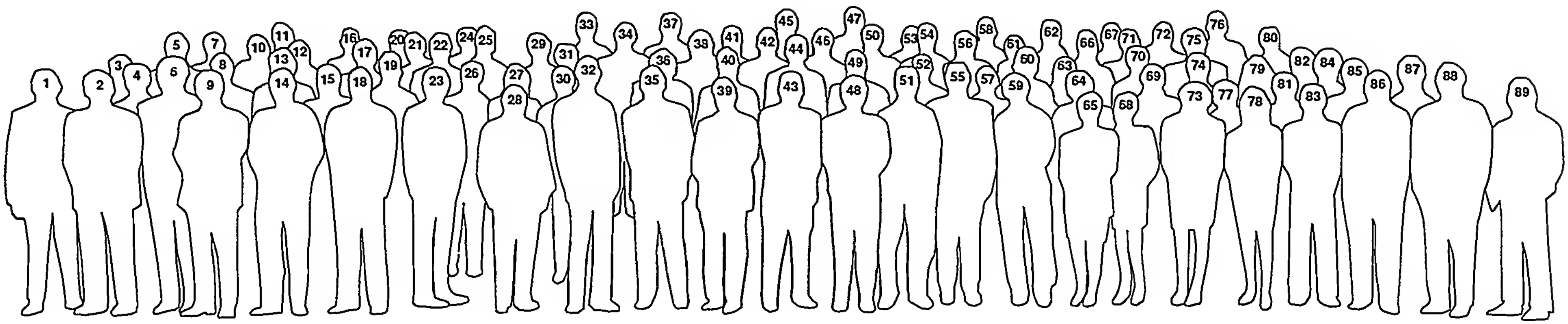

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General Containment

DID THE NUCLEAR TEST MORATORIUM OF 1958 TEACH US ANYTHING?

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ABSTRACT

The history of our behavior during and after the nuclear test moratorium could be mined to the benefit of decision makers, planners, and containment staffs. Some of the lessons learned are reviewed. Given the characteristics of bureaucracies, and the power of the concept of institutional survival, lessons of the past are not apt to be applied with much success. Nevertheless, people knowledgeable about containment are vital to the national interest, and must find a way to keep the science alive.

The decision to have a nuclear test moratorium in 1958 came about relatively quickly, but was not particularly surprising to those who were most involved, because the public pressure to do so was very strong.

The laboratories were actually saturated with test data from the 1955, 56, 57 and 58 test series, and had been on a fifty-four hour work week for so long that a moratorium as actually welcomed by many if not most. There were many laboratory staff and DoD contractors who analyzed data with considerable zeal, and good progress was made during the moratorium in understanding the strengths and deficiencies of the data taken, and in the requirements for future test, should there be more. Furthermore, as I recall, the laboratories were essentially given sufficient funds to maintain their staffs and scientific activities, so in a sense work could continue without the unbearable trauma associated with the massive restructuring that we now seem to be experiencing.

However, a very interesting phenomenon began to develop. The best people in the labs were not necessarily devoted to perpetual data reduction, especially when comprehension dawned abut data insufficiencies and shortcomings. New work involving new theories, and new modeling,

improved and advanced technologies, and occasionally a brand new capability made the future considerably more interesting than the past. When the labs realized that good people were on the threshold of leaving for more stimulating horizons, policies were established to allow the staff to work on ideas that were new and exciting, rather than on those most relevant to the basic mission. Everyone was very busy, and quite productive. The losses to other institutions and program was not too severe, and things rolled along pretty well. In fact, this spirit of well being served to keep hidden the deteriorating capabilities that the labs thought they were maintaining.

A few people within the labs had the opportunity to keep a pretty clear eye on the Soviets, and gradually came to believe that a return to testing was likely. However, it had become politically unacceptable to acknowledge such a possibility! A moratorium meant no **work** on tests, not just no test. And it was important not even to **appear** to be working on tests. As an example, Los Alamos people were discouraged from using the Nevada Test Site for any given activity, lest we be accused of preparing for testing. I seem to recall that Livermore did a few things there, but of course this just meant that they had given everybody a bad name. It is not comforting to compare the mentality everywhere rampant in 1959, '60, and most of '61 with that seen and heard expressed today, and we into a moratorium that has barely begun!

There was a policy in the Atomic Energy Commission during the last 50's and 60's of watching the change in programmatic expenditures pretty carefully, i.e., the **slopes** to the spending curves were deemed to be crucial. Surprisingly enough, the judgment seemed to be that as long as the slope was positive, that was a good program. But when the spending curve turned down, the most important question about the program became how soon it could be terminated. With growing programs defined as good, the slightest downward wiggle in spending was something to be avoided. Guess the consequences of this behavior! Good, fundamental programs were shut down! Sexy programs that might be more fun were vigorously sought. Ultimately the definition of programs changed within a give budget category. Constant or increased funding was vastly more important than the program those dollars was supporting. While this might be a viable way to

run things, it was not being done with nuclear science's best interests nor the lab's basic mission in mind. I recall during this time that our regular proposals to look into nuclear waste disposal by investigating the problem long term were redlined, that is deleted, in each budget. Few of us wanted to work on the problem anyway, so only a few people argued for such activities. Unfortunately, this experience cuts both ways. Stimulating programs are required in addition to those more boring yet fundamental to science and our mission. Perhaps it is true that you can't have one without the other. But you must find out what ball game is being played, if you are to turn it to your advantage.

Herein lies the first lesson. One must stay alive. Find out what basis is being used by the bean counters for the allotment of funds. Do not be surprised if it has little or nothing to do with the program substance. For during a test moratorium, it is manifestly clear that testing is bad, and something for which there is no justification. It will be a waste of time to imagine that policy as stated by the President or the Congress to keep alive certain capabilities is actually the one being used to disperse money. It is tempting to conclude that with people who control budgets, "Honesty is The Best Policy" is true only in the abstract. But whatever, find out what their guidelines are. Debating what they should be with your fellow workers is mostly a waste of time.

When the moratorium was terminated suddenly in August of 1961, and there arose a tremendous pressure to test something immediately, guess what? The only thing that could be tested was just what people had been working on! Those who had been working on other things were mostly, at least immediately, irrelevant! And the tests that could be done were those that the laboratories had happened to be interested in, safety tests, rather than the types of test desired by the administration. To some extent, the fault was in the labs themselves, for some there had been dutifully following the guidance and desires of Washington types, and that was to see, hear, and do no evil. But people who understood the profound questions associated with nuclear safety had continued to work diligently, had ideas that needed to be tested, and were the only ones actually prepared to proceed with nuclear tests when the occasion arose. But notice that the capability to test was a

consequence of their work, and not the object of it. So it was possible to field a safety test in about two weeks (think about that in today's climate) yet it was almost nine months before an atmospheric test could be conducted.

So to Lesson Two: Assuming you stay alive, remain devoted to the solution of the country's real problems, and be as independent of the current and fashionable political opinions as possible. Stay relevant.

I inject here my observation that CONTAINMENT is not a subject understood or appreciated by decision makers and managers, even within the labs. The first safe prediction of this circumstance is that all of you will be forced to find other jobs. But I've also observed that those involved in containment are deucedly clever, and that you no doubt will find a variety of ways to stay alive. You can be faulted only in the event that you fall victim to the belief that containment as a discipline is dead.

It was not just in the AEC that testing was believed to be dead in 1958. It also was the behavior, if not the belief, of the Armed Forces. For example, there were more than one thousand members of the staff at the Weapons Lab in Albuquerque in 1958. By August, 1961, the staff had been reduced to a number fewer than ten. So the ability to respond in a sensible way to a surprise had virtually vanished, but not really because of a conscious decision to do so. Ultimately, the decay in capability was a result of the way the budget system works. Bean counters extrapolate from past and current budgets to future ones. And so do the Congressional staffs, and managers in the Departments of Energy and Defense. Continuity and stability--these are paramount to a bureaucracy. A test moratorium means no testing, and that means than money can be saved for other things. Regular reduced expenditures leads inevitable to termination. Arguments made to the contrary are judged to be self-serving, and irrelevant. And first priorities are given to institutional survival.

Yet the kind of nuclear questions that we deal with are almost always the result of surprises! Over and over again we have found that our best calculations and simulations have failed us if and when we had the opportunity to answer certain questions. In effect, we lived from surprise to

surprise. Within the labs, this has been more or less our way of life, and we have come to regard it as a norm. But on the national and international scene, where surprises are endemic, the nation preaches continuity and stability to the degree that it becomes the only expectation. So surprises are really surprises. When one considers the capacity for surprises that can arise from elements of the former Soviet Union, and from the rest of the world, then it is clear that we're in for some fairly rough sailing!

So Lesson Three is an obvious one: you can be sure that surprises are in store! Be as prepared for them as possible, if only psychologically.

There is another observation relevant to testing that we made during the moratorium. We have repeatedly seen men and women coming to power in Washington, including both political appointees and career civil servants, who take the time to become educated to their tasks and responsibilities, and who finally come to an appreciation of the complexities involved. But few if any ever feel bound by decisions made by their predecessors. Commitments previously made have to be reaffirmed before they have validity. Conversations with Congressional staffs are vastly more important than conversations with people in the field. This places a very heavy responsibility on those in the field who are close to the national needs as defined by their discipline. They must create ways to educate the newcomers, and it must be done again and again. This in itself requires continuity in the labs, and without that, the situation is bleak indeed. But not impossible! In past years I have summed up this situation by saying "There ain't nobody there", referring to Washington, and "There ain't nobody here but us kids!", referring to those of us west of the Rio Grande. And what I have meant by saying these thing is that it is up to us to bear the responsibility to see to it that the country's needs are met. The principal responsibility is ours, and we should not expect it to be fully accepted by our superiors upon their assumption of office.

Lesson Number Four. Each of us must be part of the big picture by attempting to educate our superiors as opportunities arise. And opportunities to do so should be created, as well.

I have spoken several times about the country's needs. The question arises, how are these to be determined? Do we accept these as given to use by people in authority, say in the Presidency, or by the Congress? I mention such needs in the context of the disciplines for which we are experts. In this audience, that is Containment. We are the ones who must define the nation's needs in this field. But what standards should we use? Are they survival, relevance, immunity to surprises, education? Well, perhaps in part. But I have found a basic belief that makes relatively simple the establishment of priorities in my day to day routine. That belief is as follows: **As long as a national nuclear stockpile exists, then the country must possess the capability to address any question that my arise concerning that stockpile, and the weapons within it.** This makes clear that knowledgeable and experienced people are required, that relevant activities are essential, that experiments may be forthcoming, and a place to do them must be maintained. It is imperative that the knowledge about all phases of nuclear explosions be kept as current and as intact as humanly possible. The belief is based on the assumption what surprises will occur, but <u>it is also necessary that sufficient activities are in progress that surprises in our fields of expertise are possible</u>. It challenges us to remain active, no matter what!

In making arguments like this in the past, I have said that I know I am usually preaching to the choir. But preaching to the choir is frequently incredibly relevant, for they are the ones who are doing the singing! Unfortunately, if choirs specialize in singing only dirges, eventually a dirge makes the hit parade!

Lessons learned from the moratorium of 1958, and only a few of them have been mentioned here today, are perhaps encompassed by the following adages.

Stay cool, and relevant.

Expect surprises.

Extrapolations are always suspect. Tomorrow will not be like today. Know what you believe, and act on hat. Do no wait to be told what is important. You almost certainly already know.

Do lots of singing! And there is more at stake than just be in tune!

THE CONTAINMENT EVALUATION PANEL
Background and Foreground

James Carothers and Friends

To discuss the Containment Evaluation Panel (the CEP) we should have an understanding of what "containment" is. Unfortunately, there is no simple definition, or indeed, no uniform agreement as to what it is. Basically, it is whatever someone in the appropriate position of authority says it is, as is the case with many politically defined terms. And that also means that what it is can change from time to time.

There are two documents which shed some light on this. The first is The Nuclear Test Ban Treaty, signed on August 5, 1963 by the Soviet Union, the United Kingdom, and the United States. The operative article of the Treaty which relates to what would become "containment" as it is currently known, is Article I, Section 1.

Article I

> **1. Each of the Parties to this Treaty undertakes to prohibit, to prevent, and not to carry out any nuclear weapon test explosion, or any other nuclear explosion, at any place under its jurisdiction or control:**
>
> > **(a) in the atmosphere; beyond its limits, including outer space; or underwater, including territorial waters or high seas; or**
> > **(b) in any other environment if such explosion causes radioactive debris to be present outside the territorial limits of the State under whose jurisdiction or control such explosion is conducted.**

This seems clear enough, but there are some things that, on careful examination of subparagraph (b), are open to interpretation. The first, and most important of these, are the words " . . . causes radioactive debris to be present . . . " What comprises the radioactive debris of a nuclear explosion? Is it any radioactive product produced by the explosion, including the noble gasses? Or is it only those radioactive products which will ulimately be deposited on the ground, and thereby become "debris" — the dictionary definition of which is: "The scattered remains of something broken or destroyed; ruins." This could be interpreted as meaning that if you cannot go about the ground and find "scattered remains," or fallout particles, you have not violated the Treaty. Hence, any release of noble gasses, which dilute in the atmosphere, have no biological activity, and do not deposit, do not count. The answer to this question of interpretation is of considerable importance to the people who are charged with conducting a nuclear detonation, and at the same time with complying with the terms of the Treaty.

With one interpretation, a seepage of gasses, however large, from a detonation would not be considered a violation, no matter where or how detected, because they would not be considered "debris." Using the other interpretation, such a seepage would be a violation, if large enough to be detected outside the State boundaries.

Now consider the words . . . "outside the territorial limits of the State under whose jurisdiction or control . . ." In order for something to exist in this context, somebody has to know it's there. If radioactive material did cross the border of the State conducting the detonation, and

someone, with some instrument, did detect the activity, then the Treaty has been violated. If the material has not been detected outside the territorial limits, for whatever reason, it is difficult, or impossible to claim that a violation has occurred.

The second document that can be considered as defining containment is the Charter of the Containment Evaluation Panel. The relevant passages concerning containment itself are Articles III, subparagraphs A and C, and Article VIII subparagraph F. These are:

III A	**Emplacement and firing of each nuclear device will be conducted in a manner that conforms with United States obligations under all Nuclear Test Treaties.**
III C	**Each test will be designed to be successfully contained. Special cases will be referred to DOE/Deputy Assistant Secretary for Military Application (DASMA), for approval.**
VIII F	**Successful Containment: Containment such that a test results in no radioactivity detectable offsite as measured by normal monitoring equipment and no unanticipated release of radioactivity onsite within a 24 hour period following execution. Detection of noble gasses which appear onsite at long times after an event due to changing atmospheric conditions is not unanticipated.. Anticipated releases will be designed to conform to specific guidance from DOE/DASMA (NV-176, Revision 5, Planning Directive for Underground Nuclear Tests at the Nevada Test Site (U)).**

Note that the word "debris" does not appear. For there to be successful containment, it is "radioactivity" that is not to be detected offsite, and this term certainly includes the noble gasses. The boundaries of the Test Site are much closer to the event than the borders of the United States, hence "successful containment" is a much more rigorous standard than that given by using either interpretation of "debris" in the Treaty. Further, there should be "no unanticipated release of radioactivity onsite within a 24hour period following execution." The implication is that an unanticipated release of <u>any amount</u> of radioactivity within the 24 hour period is a failure to achieve successful containment. The monitoring equipment which might be used to detect such an unanticipated release is not specified, unlike the case of detection offsite where "normal monitoring equipment" is to be used.

Of course, from 1951, when the first tests were done at the (then) Nevada Proving Grounds, until 1963 there was no Nuclear Test Ban Treaty, and from 1951 until 1971 the requirement for successful containment was not part of the operational requirements for the conduct of a test at the (now) Nevada Test Site.

If the concept of containment is considered in a broad context, it relates fundamentally to the mitigation of the effects of the radioactivity produced during a nuclear explosion. These effects can be quite close to the detonation, they can take place at considerable distances, they can be global in extent.

Such effects began to be a problem fairly soon after tests began in Nevada. The first series, Ranger, in 1951, consisted of five airdrops, and there appeared to be no problems, except for radioactivity, deposited by a rainstorm, which was detected in Rochester, New York. The Tumbler-Snapper series in 1952 saw the use of towers for devices in the yield range of ten kilotons. By 1953, in Upshot-Knothole, airdrops were used less and less, and devices with

yields up to 32 kt (Harry), and 43 kt (Simon) were fired on towers. Both of those events caused offsite fallout problems. In the case of Simon, some offsite cars were contaminated, and had to be washed down. In the case of Harry, the people of St. George, Utah were told to stay indoors from nine until noon, to reduce exposures from the fallout on the community, and the passage of the radioactive cloud. The general public began to be aware of the actuality of, and the hazards associated with, the radioactive material from the nuclear tests at the NTS.

Worldwide attention was drawn to the dangers of fallout when the Bravo event of Operation Castle was fired on February 28, 1954. A Japanese fishing vessel, the Lucky Dragon, was some 80 miles from the detonation, and was in the fallout pattern. By the time it returned to Japan several members of the crew required hospitalization for the effects of the exposures they had received, and one died during treatment. Over 200 Marshalese and some 30 weather service personnel who were on downwind atolls were also exposed. Fallout was no longer just a local concern, or a US concern.

People working at the Test Site were having their own problems with local deposition of radioactive material. Robert Campbell was a Los Alamos Test Director at that time:

> ***Campbell:*** *After the St. George business, and after Bravo, it became obvious that we couldn't continue to have that kind of offsite fallout. If we wanted to get our job done, we were going to have to find different ways of doing things. I don't think it was the people in Washington. It was really an internal recognition that we had to do something. It was our people who were getting exposed making the recoveries. Believe me, the fallout patterns were much higher in Area 3 than they were in Utah, and we had to go in to get the data. The rule then was that you could work people forever in 10 mR fields. In Nevada I don't believe I ever went into a field that was over about 50 R. Fifty R per hour is a lot, but you could get people who had not had much exposure, and you could say, "The operation is almost over, you're going home, and this is just for one time."*
>
> *Operationally a number of things were tried, with the idea being to protect our own people. In so doing there was, of course, a benefit offsite.. They tried making large blacktop pads around tower bases, asphalt pads, to keep from entraining so much dirt. You could keep down quite a bit of the dirt that came flying up otherwise. And then there were areas where boron was put down, to reduce the soil activation. That was in about '55, or even before.*
>
> *We tried aluminum towers to get away from the steel, but they didn't work worth a damn Do you want a little steel, or do you want a lot of aluminum? There was always the business of the experimenters wanting more lead shielding, or more of something else in the cab, and the aluminum just did not have the strength and rigidity.*

By 1956 people in the testing community were beginning to consider seriously the possibilities of conducting tests underground. This was a major shift in the thinking about the problem of fallout. Previous efforts had been directed basically to dispersal and dilution; firing underground would be an attempt to control at the source, to keep the radioactivity in one place, and not to let it disperse.

Teller and Griggs in 1956 wrote a brief paper (UCRL-1659) titled "Deep Underground Test Shots". In it they concluded:

1. The cost of drilling a hole sufficiently large and deep to emplace and contain kiloton shots is comparable to the cost of erecting a tower for such shots.

2. A depth of 3000 feet is ample to be sure of no surface eruption from 30 kt and small-to-zero emanation of volatile radioactive elements. One thousand feet will suffice for 1 kt.

3. Yield can be determined within 5 to 10% by seismic and time-of-shock arrival, with suitable calibration.

4. Radiochemistry of the explosion products may be done by core drilling the molten sphere. This may be expensive.

5. Diagnostic experiments may have to be restricted to the determination of the time-dependent gamma flux.

6. Using an open hole, visual observation and interesting neutron experiments may become possible.

7. The seismic hazard to offsite structures is nil.

8. The long-term radiologic hazard is nil.

And, they recommended that in connection with the next Nevada test series, (which would be Plumbbob) a low yield shot be detonated at the Test Site "at such a depth that it will be contained."

At Los Alamos, Al Graves, head of the test effort, had arrived at the same conclusion; the possibility of doing test underground had to be explored, because nuclear tests were going to have to be done underground, if testing in the United States, at the Nevada Test Site, was to continue. No such events had ever been conducted, and the state of ignorance was vast. There were no equations of state for earth materials, and no codes into which to put them if they had existed, and by today's standards, primitive computers to run them on if the codes had existed. No one knew how big a cavity would be formed, or what the postshot cavity conditions would be. No one knew what a safe burial depth was. No one knew what the ground motion and seismic effects would be. And so on. In 1956, Graves asked Bob Brownlee to look into what might happen if a device were detonated underground.

> ***Brownlee:*** *One reason I admired Al Graves was because he was so inordinately farsighted. He anticipated problems long before other people. Where he came to have these ideas I have no idea; whether they came from his colleagues, or whether they came from the sky I don't know. One experience with testing in Nevada which must have influenced him mightily was that in '55 there was a civil defense test where they sat out over two weeks before the weather was right.*

They were very carefully watching where the fallout would go, and Al was at the center of that storm. That shot was scheduled every day for ninteen days, and Al was the one making those decisions, and taking that pressure.

He said to me, in 1956, "There isn't any doubt about it. If testing is to proceed, we're going to have to go underground. It's got to be done, whether we want to or not. Would you start working on what it might be like to have a fireball underground?" And so, I did my first primitive calculations in '56. And I actually calculated one test, Bernalillo, which we did in '58. That's how I got into the underground business, and that was strictly due to Al Graves, who recognized the necessity to go underground. There are a lot of people who don't realize that we were doing the initial work for underground tests as early as 1956.

***Campbell:** The first thing we at LASL did in a hole was called Pascal-A. It was 500 feet deep, in a cased hole. We put the bomb in the bottom of it, and we didn't stem it because we had a lid on the hole, and on that lid was an alpha detector. We wanted a line- of-sight to see if we could measure some of the reactions. So, we fired it. Biggest damn Roman candle I ever saw!*

Bad as it was, spectacular as it was, there was only about a tenth of the radiation on the ground that there would have been if we had fired it on the surface. We considered a factor of ten reduction to be wonderful; we thought we had made a real gain. A factor of almost ten meant we could get back in, get set up, and fire again more quickly. We were very happy with the results, and we did it all over again on Pascal-B.

The reduction in offsite fallout was an effect that was appreciated by the AEC, and the people who worried about offsite safety. What we were worried about was being put out of business if we had too many people pounding on the gates. And, we wanted to reduce the local fallout, the contamination of the area that we were using.

***Brownlee:** Our first underground test was done in '57. We did Pascal-A with a bomb in an open hole. Pascal-B and C had plugs, but Pascal-A did not, for other reasons. The guys had been working trying to get it ready, and there had been a number of troubles. They finally got it down hole, by my recollection, about ten o'clock or so at night. There wasn't much time to go back into Mercury, go to bed, and get up the next morning to shoot it, so somebody said, "Why don't we just shoot it now, and then go in?" So, they didn't put any plugs in; they just went ahead and shot it. And it was the world's finest Roman candle, because at night it was all visible. Blue fire shot hundreds of feet in the air. Everybody was down in the area, and they all jumped in their cars and drove like crazy, not even counting who was there and who came out of the area. I think if they had delayed another day or two they would have put a plug in it.*

Al had asked the following question, "If I take a 48 inch casing, and I put a bomb a few hundred feet down, by how much will the fallout be reduced?" We discovered it was a factor greater than ten, and almost a hundred. And that was just an open hole.

He then said, "If we put some plugs in the hole, does that cut it even further? And if so, how much?" Then he said, "Let's put a plug right down on top of the bomb, and then let's put a plug half way down. Does that make any difference?" And yes, it does a better job if you put the plug right on top of the bomb. We started exactly that way. We were still doing atmospheric shots in 1957 and 1958, so to the question of, "If you do this, or that, how much will you cut the fallout?" we could answer, "We'll measure it and see."

We saw what happened on one, and decided what to do next, but in the meantime we would have another one. So, it wasn't done quite as logically as I have indicated, but there was a thread of logic from shot to shot.

At the Livermore Laboratory, stimulated by the Teller and Griggs report, work was being done to fire a low yield device in a tunnel, with the object of completely containing all the debris produced. Gerry Johnson was the Livermore Test Director.

Johnson: *It was becoming increasingly difficult to carry out tests in Nevada because of the fallout constraints, and the public furor over the fallout. There was a rising public concern that kept growing through those years.*

Another thing was that shooting in the atmosphere required big task forces, and as a consequence we could not have continuous operations. You had to mobilize, put things together, shoot them all in an interval, then return to the Laboratories and try to figure out what happened, rework the designs, and design new experiments.

In addition to that, the operations were unduly complicated, because there were thousands of people in the field. The shots were spectacular shows, people liked to see them, and so they dreamed up all sorts of reasons for being there. That meant if you were trying to manage the operations, you had several thousand people to try to keep track of. If anything went wrong, any confusion, you had a hell of a time getting them out of there, and getting it straightened out so you could do your work.

It was in 1956 that we began to think about shooting underground. The operational constraints, which were increasing each year, were bugging us, and we were looking for a way out. We felt if we could go underground and get the data, then we could treat it as an extension of the Laboratory. We'd go out and shoot whenever we were ready to shoot, without this big task force and large numbers of people.

Then Teller and Griggs did some back of the envelope calculations and said, "Look, it ought to be possible to shoot underground, and if you had a thousand feet of overburden, you probably could shoot a kiloton or so." I was interested in it, so I said, "Well, we'll examine that. We'll get some people looking at it and thinking about it, and see what comes out of it."

Two of the big questions we had were whether you could contain it — how deep it had to be — and would the radiochemistry be any good. We had questions about the sampling. We didn't know if we'd have a pool of molten rock, or what we would to get into. There were some calculations made in terms of what you might expect in ground shock, and surface motion, and so on.

We choose the site based on topography. We decided on a tunnel geometry because we thought that would be the best way to do diagnostics. And that's how we finally ended up with Rainier Mesa. We ended up in tuff, which was good stuff to dig in, but we didn't know anything about it. We didn't know what tuff was when it was first mentioned to us.

When we were finally ready, the night before the shot we had a final review. Shall we go ahead, or is there something else we should do? And the conclusion was; everything is fine, go ahead.

We arrived at the CP early in the morning; I've forgotten what time we were to fire, but it was during daylight so we'd get good photography. I was there, and one of the members of the advisory group came up. We were about an hour away from firing. He came up and said, "Gerry, I'm nervous about that tunnel, about the containment. There are only thirteen feet of sandbags in there." I said, "Oh yes, we all know that." He said, "I'm not sure that's going to hold. Can't you just hold the shot for a few days? We'll go back in and put some more sandbags in." I said, "How many sandbags would you put in? What would you do?" and so on. Well, he wasn't sure. I said, "Well, I'll tell you. We've worked on this thing for a year. We've had the best advice we could get. If we open that tunnel up to do anything, we have to start over, repeat all our dry runs, and check everything out again. I don't know how long it would take us to get it straightened out so we could get back to a shot day. And this is near the end of the operation. We could easily lose the whole thing, administratively, and I don't want to do that. There's just no way that I can see to postpone. We're committed now. We have to go ahead. If we cancel it now we might not get another crack at it." But he really put the heat on me.

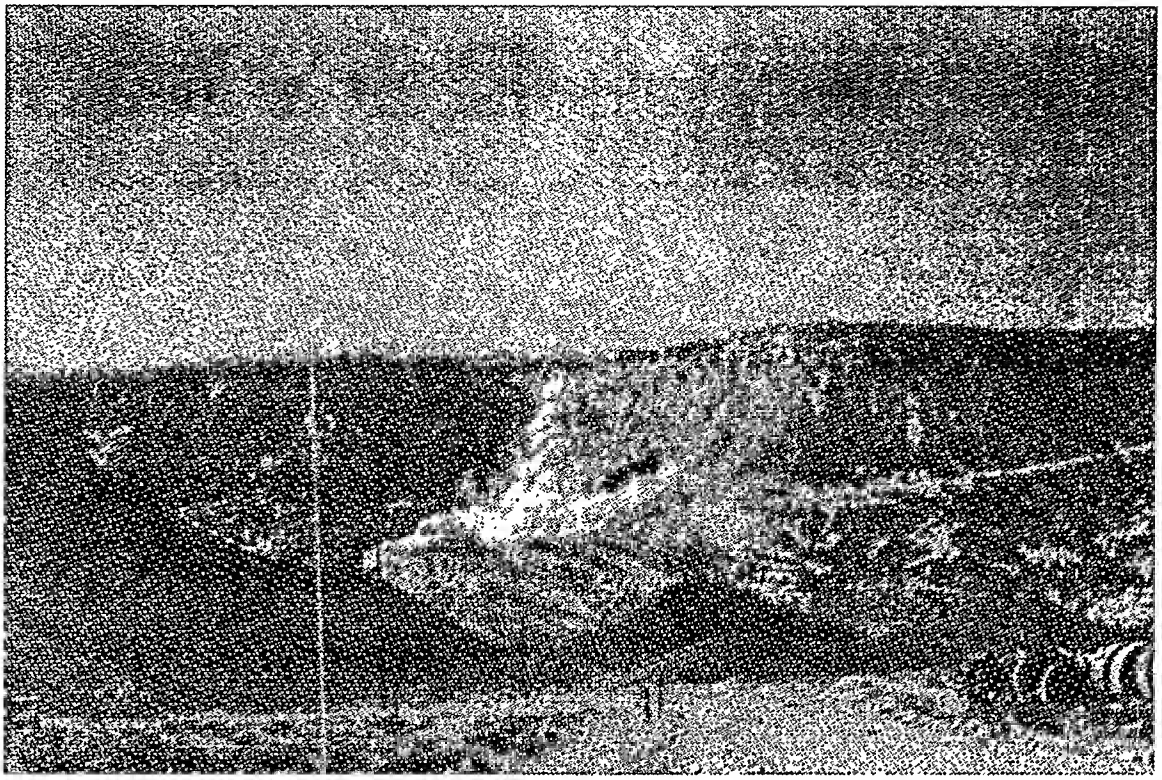
And of course, as it turned out, it worked perfectly, but that's just a bit of history, and he could have turned out to be right. But we had done everything we knew to do. We felt it at two and a half miles; that was where we fired from. The seal was just a simple spiral. We only had those thirteen feet of sandbags, and a steel door to stop gases, but the stemming worked perfectly. We got overconfident later, and had some problems, but Rainier did work very well. And, out of the Rainier shot came the Plowshare program.

Rainier was designed to be completely contained, and even by today's definitions it was successfully contained. The dragon was caged, and his foul breath no longer polluted the air. Perhaps it even seemed easy to do.

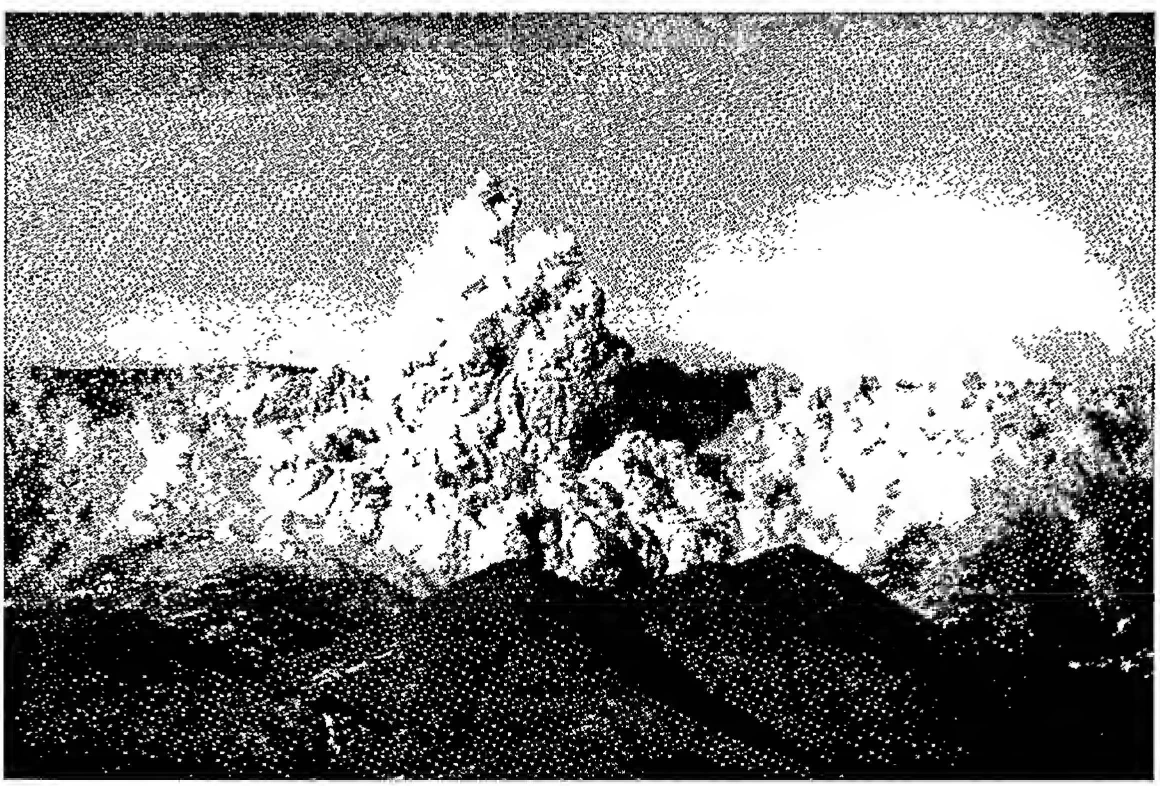
(Hubris: n. Excessive pride, arrogance. From the Greek.)

Then came Hardtack Phase II in 1958. Los Alamos conducted six safety shots in unstemmed holes, with yields ranging from zero to a few tens of tons. The events in unstemmed holes were not designed to be completely contained; the objective was still to reduce contamination in the immediate vicinity of the ground zero, and to experiment with various plug and stemming locations and configurations.

Livermore did seven tunnel events. One introduced those people interested in containment to the possibility of an unexpectedly high yield, or as some people might say, the unreliability of designers. Neptune was fired on October 14, 1958, as a safety experiment with an expected yield of zero, but with a possible yield of 10 tons or so. It was fired in a tunnel, with a working point that was under the sloping face of the mesa, with a vertical distance of 110 feet



Neptune event, Operation Hardtack II. Detonated 14 October 1958, NTS, 115 tons.



Blanca event, Operation Hardtack II. Detonated 30 October 1958, NTS, 22 kt.

to the surface, and a slant range of 100 feet to the closest point of mesa. The yield of 116 tons was unexpected, and sufficient to produce a crater. The fact that some of the radioactivity was released was not of concern; Hardtack II was, after all, principally a series of atmospheric shots, and the day before Neptune a 1.4 kt device had been fired while suspended from a balloon. The Livermore people, showing admirable flexibility in their thinking, promptly called Neptune a nuclear cratering experiment, and in a future report (UCRL-5766 The Neptune Event; A Nuclear Cratering Experiment) discussed the "major contributions of the data to the theory and prediction of cratering phenomenology."

Two of the tunnel events were designed to give appreciable yield. Logan, fired two days after Neptune, produced 5 kilotons, and was successfully contained. Blanca, fired on October 30, 1958 produced 22 kilotons, and like Neptune, vented out the face of the mesa.

The Logan event was interesting for several reasons. It was an effects shot, in a tunnel, with a horizontal vacuum line-of-sight pipe which extended for 150 feet from the device, opening to two feet in diameter at the far end. From there two six inch diameter pipes extended another 75 feet. The design team started with some money, very little Laboratory manpower support, some contractors, a pad of blank paper, a tunnel that was still being dug, and six weeks to design the experiments and the diagnostics, fabricate the hardware, and have everything installed for the shot. That is an incredibly short time scale by today's standards. And, Logan was successfully contained, even without the CEP to help with a review of the containment features of the event. Arnold Clark was the project physicist for Logan.

Clark: *We had six weeks, because we had to shoot two weeks before the end of October. We were going to shoot in a tunnel — which hadn't been finished being dug yet — where an important shot, Blanca, was going to be shot in another part. So, they had to have two more weeks after we shot to finish off the cabling for Blanca. They would finish digging out a side drift place for us, and they'd pull cable for us. Our biggest problem was that we wanted a vacuum pipe in the tunnel. Here we were, starting with a blank piece of paper, and we had five weeks to have that pipe finished, installed, and pumped down.*

They said, "How long do you want it?" We looked at our blank piece of paper, and said, "A hundred and fifty feet." "How big around?" "Oh, about this big." And that was the process we went through to specify it. So, we had a 150 foot vacuum pipe, maximum diameter of two feet, made by NRL in Washington. It was flown out, installed, and evacuated. And it held a vacuum! In five weeks!

Lockheed made a very fancy, very strong steel sample holder to put at the 150 foot station. Then people had second thoughts about that station, and said, "That is not going to survive. Or maybe it's not going to survive." They didn't know. "Maybe we better go out farther." So, we extended the pipe to 225 feet by adding two pipes, 6 inches in diameter, to the back end of the big one, to go out another 75 feet. And that's all that survived; the 225 foot stuff. We never saw any of that 150 foot station after the shot. That was where the container of very special steel, made by Lockheed, had been. It was a huge thing, about the size of a really good-sized safe, just essentially solid steel. And it was a very special steel alloy that was supposed to survive. Well, it didn't. There was very little from that station.

We had a quite elaborate closure on the front end. There was a very fine theoretical physicist, Harold Hall, working for Montgomery Johnson in early '58. They were worrying about this containment problem, and Harold came up with

the idea of a Box A type closure, as they call it now. This was a brand new idea. Harold Hall did some calculations, and so did Montgomery Johnson, and they said, "Ah, yes!" So a Box A type closure was used for the first time on Logan, and it worked very well. I think the front end was a foot in diameter, which is pretty big. Maybe it was ten inches.

When they were digging back after the shot they also drilled back at different areas around the zero room, and found that the really highly radioactive area, I guess you would call it the cavity today, was pear shaped. It wasn't circular. Some activity had come down the tunnel, but not very far except for a few cracks that went out as much as 150 feet. So, it did contain completely.

However, it went a little larger than the designer said it would, and it knocked in the side of the tunnel where Blanca was supposed to be. So, Blanca was not shot where it was planned. It had a beautiful alcove way back in the tunnel, but the side of the tunnel was shoved in, and in two weeks, what do you do? You put it nearer the portal, wherever you can find a place.

So, Blanca, instead of being shot underneath the mesa where it was supposed to be, was shot beneath the very steep face of the mesa, out where the overburden was maybe half of what it would have been. I watched it, and I thought the side of the mountain was going to come right towards me and hit me. I was only two miles away.

The 1958-1961 moratorium followed Hardtack II. During the moratorium Los Alamos drilled some stockpile holes in Yucca, and LRL continued with excavations in B-tunnel, and E-tunnel. Considerable reentry work and explorations were done on Rainier. But, little known until recently, a series of experiments took place which contributed to the knowledge about containment. Bob Brownlee describes his work during the moratorium.

Brownlee: *There was something that went on during the moratorium which used to be supersecret but isn't anymore. There have been announcements about it, and newspaper stories. That was a series of one-point kind of experiments, and so we had a rather active underground experimental program here at Los Alamos. You didn't see towers, and you didn't see smoke, and you didn't see a lot of things. But out in TA-49 we put things down holes, and fired them. It was during that period I saw my first stemming falls, from a whole series of those things. It always happened. We'd shoot one of these things off, and a little while later the stemming would fall down the hole. We were doing them in tuff, so the holes tended to stand, and the stemming would go down. So, it was during the moratorium that I began to appreciate cavities and chimneys.*

Bob Newman and I spent an appreciable time fussing about scaling laws. How big a cavity would we make? How much stemming did we have to have to keep everything contained? The difficulty was that the number of people who knew about that program in Los Alamos was minimal. In J-division there was Westerfelt, Newman, Campbell, and a few others, including myself. And of course, in W-division there were the people who were making the devices.

So, I continued to get an education in containment during the moratorium, which if you stop to think about it is odd. But it was kept so close that only Campbell and Newman would talk to me, and they didn't talk to many others at all. I was not allowed to know very many details. The part of it that I knew was that we

were doing things that required stemming and containment, and we didn't dare make a mistake. It had to be contained, and we had therefore to be super-conservative. It wasn't like the Test Site. If something happens around here in Los Alamos, everybody in town knows it. There's no way you can hide it. The argument was that we didn't dare go to the Test Site. I thought that was a bit odd, but that was my understanding. We had to do it here because the Russians would know we were doing something if we went somewhere else.

So, at Los Alamos we were learning a little something about underground containment. We talked a lot about scaling laws. We debated whether we needed a depth of burial where there wouldn't be a crater, or what it was we did need. My recollection is we kept debating what it meant, but with people like Campbell in the works those kinds of subtleties were often scorned. Obviously what we meant was that nothing comes out. So, at those very early times we had already, in a way, defined containment as not one atom out. There was nobody who told us to do it that way.

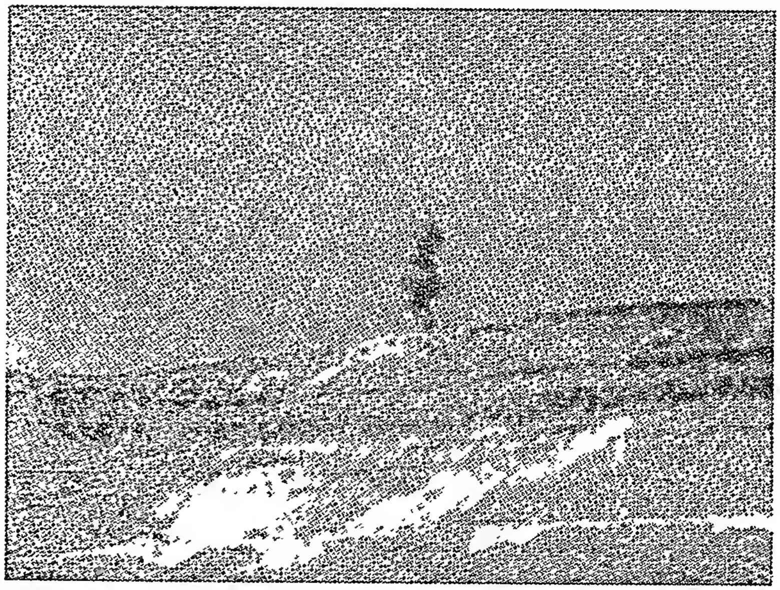
These scaling laws you could find in the literature were, of course, for chemical explosions, which is actually what we were dealing with, in a practical sense. So they were relevant, in a way. As a result of all that we came to '61 with the conviction that 400 W to the 1/3rd, in feet, was conservative, and worked.

In summary, I would say that more happened during that moratorium that's relevant to containment than you might think. Even though it was hidden, and there weren't very many people involved, there was a continuation of thought. I think we were more ready to test underground than people remember.

The test moratorium ended in September of 1961. Following the atmospheric detonation of a Soviet device with a yield of over 50 megatons as the first of a series of Soviet atmospheric tests, President Kennedy ordered the resumption of testing at the Test Site. There was the proviso that the US tests should be carried out underground, unless a specific exception was approved. The first event at the NTS was the 2.6 kt Antler test, fired on September 15, 1961. It was followed by Shrew, a safety test in a drill hole, fired on September 16, 1961. Both events released measurable amounts of activity — the activity released from Antler was detected offsite, that from Shrew was not.

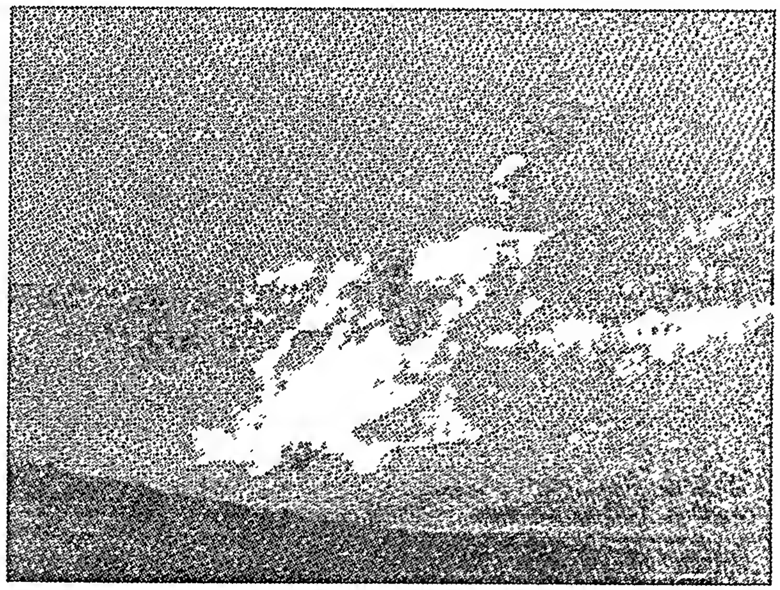
During the next few months the experience of both Laboratories showed that the containment of the radioactive materials produced by an underground detonation was not a trivial task, whether the device was emplaced in a tunnel, or in a drill hole. During the first year there were 43 shots fired in emplacement holes. One, Eel, released some 1,900,000 curies, and the activity was detected offsite. Twenty-one released material that was detected only on-site. Twenty-one are not recorded as having released activity. These 43 events included those conducted by both LASL and LRL.

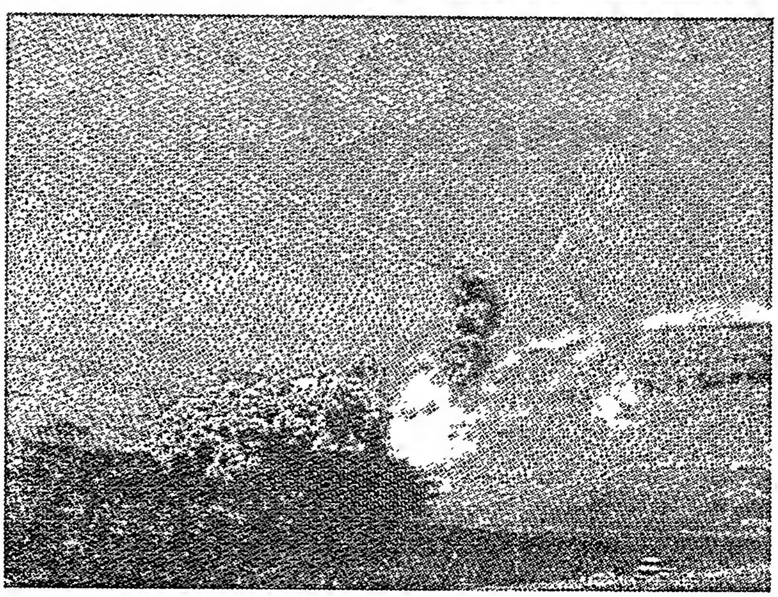
Livermore fired six devices in tunnels, including the Gnome experiment in New Mexico, and all released activity, some in spectacular fashion. The following pictures of the Des Moines venting were were taken by the author with a hand-held camera. The pictures were taken at irregular time intervals; the elapsed time between the first and last is probably about ten minutes. The total release is recorded as 11,000,000 curies, which is the largest release from any underground event. Regardless of what definition is chosen, Des Moines was not successfully contained. It is instructive to observe the amount of material ejected from the tunnel by the energy release from what was a rather low yield device.



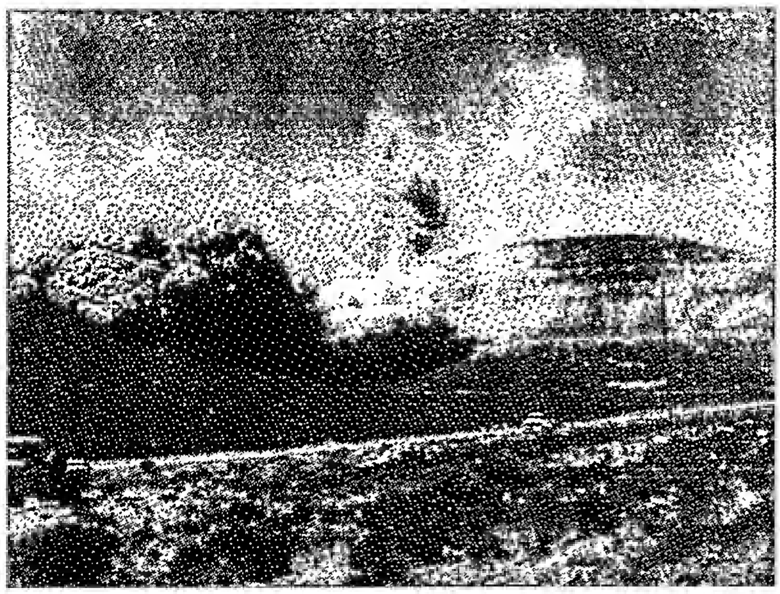
Des Moines, a few seconds after zero time. The plume at the top is from a hole drilled to allow the collection of a prompt radiochemistry sample. It was prompt all right. Fast photo records show the first arrival within one millisecond.

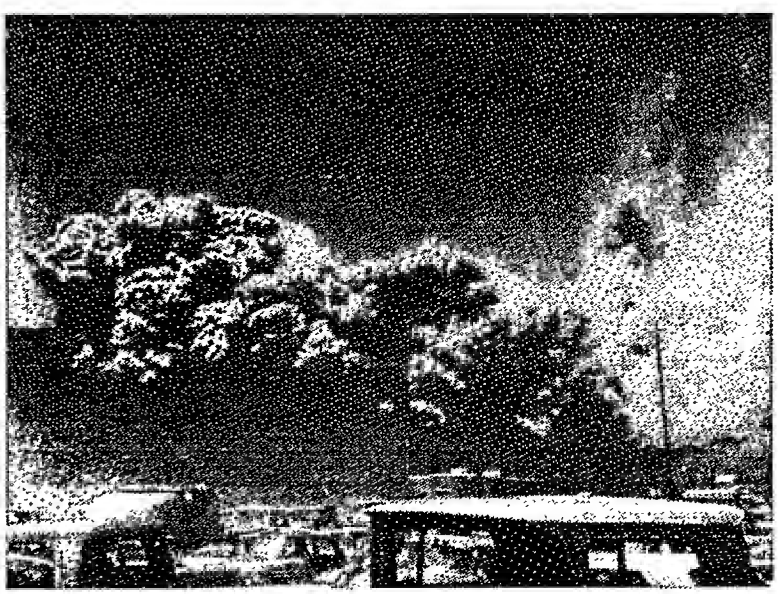
The plume on the face of the mesa was from a hole drilled and left open to vent material that got into the main drift, in order to relieve pressure on the gas seal door. It didn't work. The small cloud at the lower left shows the door is gone and material has begun to vent from the portal.

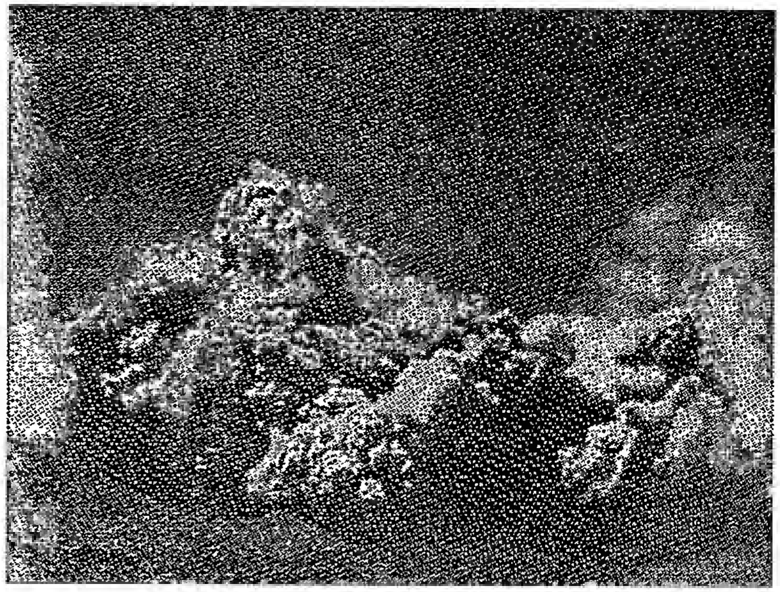




The debris venting from the portal stayed close to the ground as it moved up the small hill to the left, as shown in the following three photos.

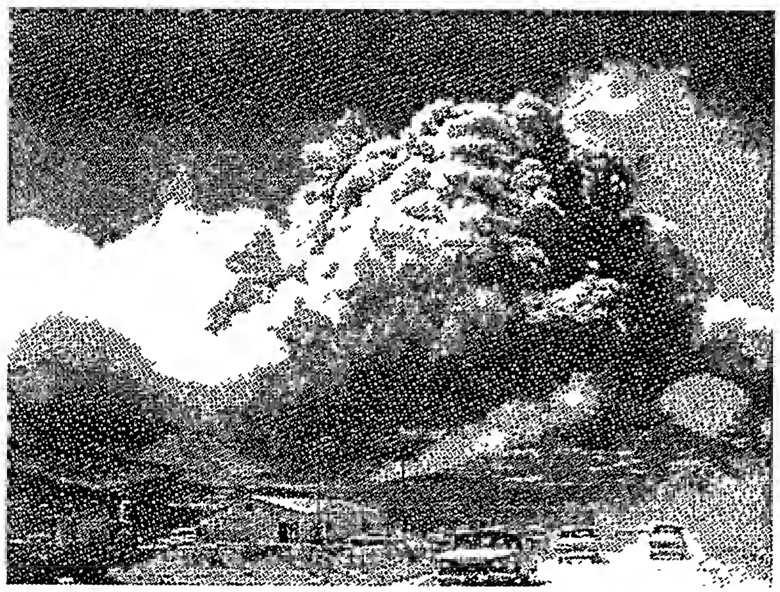


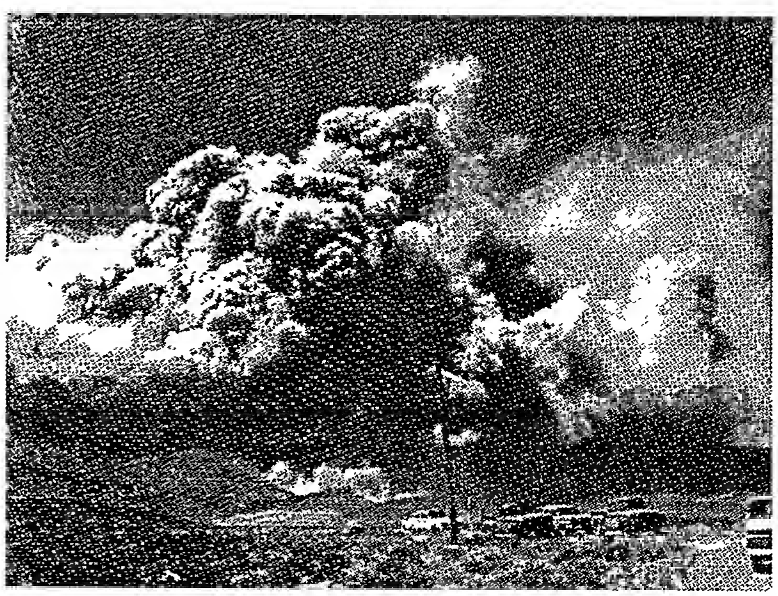




The cloud of material from the portal continues to expand, and is approaching the place where a few hundred observers were standing.

By unspoken mutual consent the observers decided to head back to the CP for a cup of coffee.





Perhaps ten minutes after zero time. Taken from about a mile distant from the observation station.

However, the Des Moines venting, or that of Platte (about 2 megacuries), or that of Eel (about 2 megacuries) did not cause significant problems to the overall test program. It was a problem to the people at LRL, but there was no stoppage of testing while the causes of the venting were explored, there were no reported radiation exposures, and so on. Now, Des Moines was detonated on June 13, 1962, and the Dominic operation was actively being carried out in the Christmas Island area. In the week preceding and the week following Des Moines, there were a total of seven airdrops of devices of intermediate or low megaton yield. If 10 megacuries is taken as the H+12 hour activity from 1 kiloton of fission, the Des Moines release was about that of a 1 kiloton atmospheric shot, which was trivial compared to the activity being released in the Pacific, and perhaps that influenced the AEC. On the other hand, it was close to home, and the people whose data was lost were not happy. LRL fired only two more tunnel events after Des Moines. One, Madison was the only tunnel shot LRL fired after the moratorium that contained. The other, Yuba, fired on June 15, 1963, was not contained and released material that was detected off the Test Site.

It was in 1963 that the Nuclear Test Ban Treaty, widely known as the Limited Test Ban Treaty, or the Partial Test Ban Treaty was signed. As observed above, the Treaty did not say any event had to be contained — only that the radioactive debris should not cross the border of, in this case, the United States. Examples of the application of that requirement is the fact that cratering shots continued until December 8, 1968, when the 30 kt cratering event Schooner was fired, presumably under meteorological conditions that would retain the vented activity with the boundaries of the United States for some indeterminate time.

So, even though the Nuclear Test Ban Treaty had been signed, containment failures, as defined today, continued to occur. Many of them were minor seepages, but some were major failures, particularly for the experimenters.

One in particular, the Pike event, fired 3/13/64, has had a continuing and major impact on the Test Program. The fallout projections for every shot today are based on the "Pike Model," which means that the yield of the proposed shot is scaled to the readings that were obtained in the Pike fallout pattern. The basic assumption is that the proposed shot will release the same fraction of the activity that Pike did. Bob Brownlee explained his views on that:

> ***Brownlee:*** *Pike has cost all of us enormous amounts of time, and effort, and money, and I think needlessly. That is a thing I have never been able to communicate to NVO in modern times. You see, one of the things everybody forgets is that we had an x-ray LOS pipe on Pike. It didn't come to the surface, so people forget that it was there. And, since it didn't come to the surface, although it went a substantial distance, it had no closures or anything. That pipe was one of the key factors. Another was that Pike was expected to have a max cred yield of a certain value — not very large, but definitely not a safety shot. Well, it went over one and half times the max cred. And then, it was in a very shallow hole; 400 feet or so.*
>
> *What I knew about it was that it had this predicted max cred, and the pipe was so long and so big. I said, "A lot of energy is going to come to the top of that pipe." I did not realize that there was any chance that the yield could go higher than what I was told, or I would have hollered. I knew that it was in a shallow hole, so that bothered me as it was. Another thing I did not know was that when they drilled that hole they had run into this hourglass sand. And guess where that layer of sand was — which I found out after the shot. It was right at the top of that pipe.*

Now, there's no chance in the world we will ever duplicate Pike. First of all, we won't shoot anything at 400 feet. Secondly, we won't have a pipe on it like that. Thirdly, in a medium where the sand was running like water — we'll never do that. And finally, having a max cred yield as bad as that is kind of unthinkable. I say, "Kind of unthinkable." When you combine all those things, Pike was a lead-pipe cinch to be spectacular.

My argument is that to treat every shot like Pike is absurd. It's just absurd. There isn't anything that's going to vent like Pike, because we don't do those things anymore. We will never duplicate Pike, and yet we pretend to the world, and to society, and to the President of the United States, that this shot we're considering could come out like Pike did. We don't intend to communicate that message, but that's what we do, and <u>it's just not true</u>. It's not going to come out like Pike, because Pike had too many great oddities.

Pike is what really brought us to every detail going through the hands of the containment people. We said, "No more are we going to take anybody's word for anything." The Pike experience was profound for us, because that's when we realized that no one person was knowledgeable about everything on a shot. I was responsible for certain things, but not everything. After Pike we began to function in what I'll say is a modern way. We had a meeting in which the bomb designer had to come and swear he knew what the yield would be. That really came as a result of Pike. Before, it was by chance. You knew what you knew by who you happened to talk to, but if people were on vacation, or you were on vacation, you didn't talk to them, and you didn't know whatever it was they could have told you. I learned a bitter lesson on Pike, which was that I thought I knew what the shot was and I didn't. I didn't know the yield, I didn't know the geologic setting, and I didn't know about the sand. All these things came out in the wash.

The first political fallout was about the fallout. It was on Las Vegas, and it also went straight toward Mexico City. I think somebody in the embassy read something and didn't know what he read, and it never really did get reported in a sensible way. I don't believe you'll find any record of a measurement having been made in Mexico City, but I believe there was.

But, we had violated the treaty. It had crossed the border. So the first slap on the wrist was, "You fired in high winds that were aimed right towards the border." And that's true. "Why did you fire in high winds?" Well, that wasn't my responsibility, but I was digging back through the data because I knew we were in real trouble. Then Al Graves had a meeting, and I went, and Westerfelt, and for the first time we put together all the things that were wrong. And I was appalled. So we, at the second level, bared our breasts and said, "Well, we had done this, and we had not done that, and the yield was quite a bit higher than we were expecting, etc." Then they came down hard on us on all those points, but they knew about them because we told them.

The thing they knew was that it had crossed the border. We made lots of promises of brand new procedures, which indeed we did initiate. And we really changed our working relationships after Pike.

The Test Evaluation Panel came into being in 1964, perhaps as a result of the Limited Test Ban Treaty, and of Pike. The Panel consisted of persons furnished by LASL, LRL, Sandia, DOD, the Public Health Service, and the AEC, and individual consultants. The USGS furnished the geologic information. The purpose of the Panel was to "review data associated with proposed nuclear tests or demonstrations in terms of past experience, probable violations of the treaty provisions, and current detection techniques."

The TEP had three categories, much as the CEP does, but there the similarity ends. The TEP Category A was "An underground test which, on the basis of experience, should not vent significantly. It must be understood that even in this category unforeseen conditions may develop which result in the release of detectable levels of radioactivity at the border." The NVO Planning Directive for 1964 said, "The emplacement and firing of devices will be designed to result in containment in all cases where this requirement is not inconsistent with the technical objectives." Cliff Olsen made these comments about the TEP:

> ***Olsen:*** *The people who tended to be at the TEP meetings were the Test Group Directors, and they presented the shots. The presentations were very rudimentary. There was a data sheet, and maybe a line-of-sight pipe layout, or a stemming drawing. Often there was no stemming drawing, because we had generic stemming plans. There was LASL 5, or LASL 2 at that time. We would have our stemming plan, which was pea gravel with fifty feet of sand halfway up, and fifty feet of sand at the surface, and that was our stemming plan. So, there was no need for a drawing, because they were all the same. The TEP got into reviewing designs a lot more than the CEP does. In a sense, they would suggest changes, and they would actually review mechanical designs — why don't you do this, why don't you do that. And design changes were made as a result of the TEP.*
>
> *One of the things with the TEP, which I guess was sort of indicative of the climate at the time, was there were three categories — A, B, C — and C was, "We expect it to leak." There was no particular onus to getting a C. That was simply it. And it simply meant that you made different notifications before you shot it. It had nothing to do with whether you were going to execute the event. It wasn't that somebody in Washington or Germantown was going to have a hemorrhage when he saw it. It was just the design of the event.*

The Livermore Hupmobile event, fired on 1/18/68, released activity that resulted in a major loss of data, and radioactivity was detected offsite. It did not result in the kind of long-lasting operational changes that Pike did, but it was significant in that it led to the formation of a separate containment group at Livermore. Cliff Olsen and Billy Hudson were members of that group.

> ***Olsen:*** *Hupmobile was a disaster. The x-ray people wanted collimators, because they did not want shine bouncing off the walls, so we put in collimators at almost every pipe joint. This was a fairly large line-of-sight — it went up to several feet in diameter at the surface. We had these relatively massive collimator rings, and for ease of installation they had a little, very thin, metal lip around the outside. So, they just sat on a pipe joint, and there was virtually no strength in the thing that attached them to the pipe.*
>
> *When the flow came along, going upward, and started dumping energy on the downstream side of these things, this little rim of fairly thin metal that was holding them in place gave way. So, these rings went up the pipe, became a*

tangled mass of stuff at the top, and blocked all of the valves. We recovered, on reentry, something like sixteen hundred pounds of twisted up collimator rings at the top of the line-of-sight pipe. We could even identify which collimator it was that had been torn loose.

There was a transient ground shock closure at the bottom, and it took like twenty-five seconds or so for the cavity to find the weak spot and erode it enough that it really blew its cork. There was a good sized cloud, but the flow was going through the pipe, so it didn't erode as much dirt and dust as Baneberry. The release was smaller than on Baneberry, but I think it was within an order of magnitude. It was big.

We had a large, several story exposure station at surface ground zero, on top of the pipe, and because of the venting that large exposure station caught fire, and we lost a large share of all the things that were in it. The experimenters didn't like that at all. In those days I think ninety percent of the reason for expending effort on containment related to data loss, rather than pressure from Washington.

That begin to change probably around '67 to '68. It may have been as a result of Hupmobile, because the people in Washington who supplied the money, even though they were not so worried about the loss of data as the experimenters, got antsy about dumping money into these things and not getting anything in return. Hupmobile was quite expensive for the time. I think that may have been the first thing beyond strictly experimenters wondering why their film was black.

***Hudson:** Hupmobile turned out to be a containment fiasco, in that a lot of the film data was lost due to the radiation release. The decision was made at the Associate Director level to form a containment group, and try to put some serious effort into understanding containment and saving the film. In any case, Jim Carothers asked me to join that group, and it appeared to me to be an offer that I couldn't refuse.*

I believe it was in late 1968 that happened. For about the first two years Bill McMaster and I used to have some words now and then about how this surely wouldn't be more than a two year problem, and then we could get back to doing some science. "We'll figure this out, won't take more than two years, then we'll get back to interesting physics." Fortunately, it got more and more interesting, because it turned out to be much more than a two year problem.

The primary problem then was to protect the film so the prompt diagnostics folks could go back to the Laboratory, read the film, and tell the bomb designers what they did right or what they did wrong. It was not to protect the environment; it was to protect the film.

The Partial Test Ban Treaty had been signed in 1963, several years earlier. The Treaty said we were not to do any experiments where radioactive material would go beyond the national boundaries of the US, That was a primary guideline. No radiation across our international borders. But in fact, measures had already been taken to pretty much limit the escape across the border. Just by the act of putting a few hundred feet of dirt over the device, you almost always eliminated radiation getting to the border. There were a few events after the early sixties

that let some material across the border, but they were very few. It was mostly a local problem, because the radiation leakage would be confined almost to the site of the event itself— maybe a little larger. But, it was that local radiation that was causing the damage to the film, and it was the kind of problem most often encountered.

If we got up to the neighborhood of ten thousand curies there was a possibility of activity getting offsite. Less than a thousand curies was of little or no concern to the general public, or the people in Washington. However, it was of great concern to the people whose film was in the recording trailers.

In the late 1960's, early 1970's, they were doing some exposure experiments, with an open line-of-sight pipe to the surface. It took a few tries before the hardware was properly designed to stop the rush of hot gases and refractory products to the surface, but that problem was pretty well solved by the time the containment group was formed. I don't think people realize it, but we didn't see much more of the Hupmobile type releases on our line-of-sight shots after we formed the containment group.

Baneberry was the watershed event in the history of containment. Detonated on December 18, 1970, it vented spectacularly some three minutes after being fired. The cloud of dust and debris rose some 12,000 feet, and was reported to have been seen by people at the NVO offices in Las Vegas. The total release is today given as 6,900,000 curies (H+12 hours). Interestingly, there was almost no fallout from Banebery. The integrated total activity in the fallout pattern was about one percent of that in the Pike fallout pattern. The Area 12 camp had not been cleared before the shot, and surface winds carried some of the activity in that direction. During the time it took to alert the people there, and to clear the area, a number of people received radiation exposures, and some of those filed lawsuits in the following years, alleging damage to their health and longevity.

The AEC allowed no more detonations for some six months while a committee, called the Vincegerra Committee, after the Chairman, examined the causes of the venting, and the method of operations at the Test Site. In the report of the committee several recommendations were made for changes in the way future test operations should be carried out, and how improvements could be made in the way the containment aspects of an event were evaluated. One of the recommendations was that the Test Evaluation Panel should be reconstituted, and a new Charter developed for the new Panel. The Containment Evaluation Panel, as the new Panel was called, consisted of one member, and an alternate, nominated by each of LASL, LRL, Sandia, DNA, USGS, and the Desert Research Institute. In addition, provision was made for the Manager, NVO to appoint one or more consultants. Members nominated by particular organizations, or consultants recommended by the Chairman, were formally appointed by the Manager, NVO, to serve on what was an advisory Panel to him. Carter Broyles, Sandia, was one of the first members of the CEP.

***Broyles:** I think the members of the Panel all recognized there was a political need to be met, to prove to the nation that we were paying attention. That contributed to the CEP being set up. And clearly it was evident in the series of proposed charters, and hassling that went on between Nevada and the Labs and Washington on just what the charter should say. I think I viewed from the very beginning that the CEP took its role as a technical judgment body seriously, and more than just political window dressing.*

In fact, I think some members perhaps were over-enthralled. Not so much over-zealous, but perhaps they did not have a full appreciation of the limits of our technical knowledge, and therefore tended to give themselves more credit for how sure they were of any technical facts than we really were. They didn't necessarily recognize the technical limitations, and the lack of knowledge of geophysics and geo-engineering, and what the characteristics of the real world were, how variable they were, and the limitations of the calculations.

Clearly various parts of the structure looked different from different perspectives, and the CEP, I think, was many different things to many different people. But the Panel itself, from the very beginning took its role seriously, and took it as a technical challenge to do the best job they could, recognizing that the world was going to be different after Baneberry.

Recognizing the changes that were taking place, and the strong requirements that were being developed for complete, or successful, containment, Los Alamos organized a formal containment group. Bob Brownlee had been working on the containment of underground events since 1957. In 1966 he was joined by Carl Keller, and they did a number of calculations and experiments related to line-of-sight shots, but it was not until after Baneberry that a containment group, per se, was formed. Jack House, Los Alamos, was one of the people brought into this new group.

House: *Bob Brownlee and Carl Keller had been working on underground shots for some time, but there was not a formal group until after Baneberry. Bob and Carl were in group J-15, as I recall. Bob was the principal containment person, and Carl was working with him. It was very early work, where they were trying to keep shots, particularly pipe shots, from releasing radioactivity to the atmosphere.*

About two months after Baneberry I got a call from Bill Ogle saying that I was being temporarily reassigned to a new group that was being formed under Bob Brownlee. It was to be a containment group called J-9. At that time there were only three, or maybe four staff members at Los Alamos who had earth science degrees. I have a bachelors degree in geology from the University of New Mexico, with a civil engineering minor, so I got drafted into J-9. The people who were in J-9 were Brownlee, Sharp, Keller, myself, and a few other folks, probably less than ten altogether, that Brownlee had assembled from other groups in the Laboratory. So we had this little cadre of dedicated personnel who were to do "containment." Whatever that was. I didn't know anything about containment, except probably how to spell it.

At that time, in early 1971, we were in the six months test moratorium mandated by the Atomic Energy Commission post-Baneberry. All the emplacement holes that Los Alamos had in inventory were cased. Well, how can we do site characterization and examine the material properties in a cased hole? So, we initiated a drilling program for exploratory holes, in close proximity to the emplacement holes, that could be sampled and logged. Because we didn't have an adequate staff or the necessary expertise, the data went to the USGS at Denver, where Evan Jenkins and Paul Orkild, and their staff, did the analysis. They would then put together a site characterization package — the cross sections and the whole nine yards. Livermore at that time was able to do those kinds of things in-house, because they had the necessary personnel.

It was a real circus in those early days of 1971 while we were still learning the containment business, We didn't have any designated presenter for the events that came before the new Containment Evaluation Panel, as Livermore did. The very first event that was presented by Los Alamos to the CEP was a shot in Area 3. Bob Brownlee sat at the CEP table and read the prospectus to the Panel. I was sitting in the audience along with essentially all the rest of J-9, there being only a few of us, and that's how the presentation was made.

The DNA had taken a different route than the Laboratories in approaching the problem of containment. They were doing both vertical and horizontal line-of-sight shots, with limited success in containing the radioactive products of the detonation. And, like the Laboratories, they were losing experimental data. Joe LaComb, DNA, had much of the responsibility for the way the events were designed and constructed:

LaComb: *By 1966 we cared about containment, and I cared about it, because it was the same as it is today. If we don't keep it in close, we don't accomplish what we want to accomplish. We lose our experiments. On Double Play, which was in June of '66, after we had problems with Discus Thrower, Red Hot, and Pile Driver, Jack Noyer came out and said, "How long will it take you to build an overburden plug?" So, we built an overburden plug in five days. We put that plug in because our containment record wasn't very good. We already had a gas-seal door in the drift, so when we ended up we had, in general, the same kind of configuration we do nowadays, although we didn't have a lot of the things we do now, like cable gas blocks. That plug was strictly for public safety and health. It wasn't going to help our experiments at all.*

I don't think at that time DASA had anybody who was designated as the person to be concerned about containment. There wasn't really anyone who was given that responsibility, other than Jack Noyer. Being the kind of person he was, he tried to worry about it all. The person I listened to was Wendell Weart, from Sandia. He was the one who came out when we had questions regarding how should the stemming be placed, should the hook drift be left open or should it be backfilled — those kinds of things. Mel Merritt was another one who helped.

It was about that time that they started doing calculations with a bunch of folks who were with General Atomics. It started out with some GA folks involved, and there was, right after Double Play, some RAND people involved. For Door Mist, in '67, it was those contractors who were doing the calculations, and were saying we want this kind of grout with this kind of strength, and so forth. As far as the overburden plug and the gas-seal door went, that was more or less our engineering problem. There were no real criteria.

So, there wasn't anybody in DASA, in the Door Mist time frame, saying that it was this or that, that DASA wanted. The contractors were saying, and saying more or less directly to myself and the Test Group Director, "This is what DASA wants." Somebody said they wanted a plug, or particular kinds of grout, but it wasn't my job to define those things, only from the standpoint that I tried to make sure we got the materials that the "experts" thought they wanted.

Right after Door Mist, where we had more problems, Noyer told me, "That won't happen again. You're going to take care of this." I said, "Yes sir." We could see we were going to have to pay some significant attention to protecting the experiments, and to stopping leaks. It was, in our program, always a constant threat that the people funding the experiments would decide that the possibility that they would lose a lot of their data was too big to take a chance on. That's one of the reasons they tried to turn things around so rapidly after the four in a row in '65 and '66. I don't think it was so much a big concern about the fact that we were releasing a little radiation to the atmosphere. It was the loss of the experiments, and our credibility. I think that has always been a factor.

About that time we formed what we called SACPAN Junior, which was a working group. It was a real mixture. There was Court McFarland from headquarters DNA, who was an aeronautical engineer, but very interested in materials behavior. There was Ben Grody, who had a doctor's degree in geology, and Bob Bjork, who was, and is, an excellent physicist, myself, and Jerry Kent. That group, in my opinion, really turned our containment program around. I think that group was the real foundation of the DNA containment program.

When Baneberry happened we were working on Misty North. We were also getting ready to field Diagonal Line. We had to go and present a risk-benefit analysis for the Misty North shot to get permission to fire it. And we also had to do that for Diagonal Line. There wasn't a whole lot of confidence.

We also had the presentations we had to make to the CEP, and there were problems with some of the early shots when we presented our material, in getting our ideas across about what we were trying to do. I was talking to Carl Keller one day, and we were kicking ideas about this problem back and forth. He said something about vessels, and I said, "You know, it might be worth thinking about that." So, it was when he and I were talking that the seed was planted, I think. I decided there had to be a logical way to present this material, so you could say, "That will be coming in this section here." So, I sat down and I said, "Okay, we've got three vessels," and I started writing up the presentation based on the three vessel concept. I made out the outlines, and then went back and started writing how you would do it. They're still using some of the same words today.

In '74 DNA hired Carl Keller to be the Containment Scientist, and early on he began to develop an experimental and calculational program to try to understand some things about what was going on. Through the years thirty to forty percent of our effort with Pac Tech and S-Cubed has been in research, to do something new and different, to find out something. We've got to do our production work that's associated with the test, but it's essential to me that we still keep enough effort in there to try to find out if there isn't something there, something we're missing. I always have the feeling there's a shadow lurking around the corner.

In July of 1974 Carl Keller, who was Los Alamos at the time, was hired to be responsible for the DNA containment program.

> ***Keller:*** *I think the title of the position was Containment Scientist, and there was a job description associated with that, as the Civil Service requires. That job description had been developed by Jay Davis, my predecessor at DNA by more than a year, and by the Director of the Test Directorate. This was a new job description, with their new concept of what the Containment Scientist ought to be doing.*
>
> *At that time DNA had numerous problems, and they had decided that they needed a heavier gun in the Containment Scientist position. They upgraded the position from a GS-14 to a GS-15, which meant they could offer more pay. I know that they had solicited several senior people in the containment business to take that job — people far more senior than I was. Those people, I suspect, were already above that pay grade, and probably well established in the Laboratories. I wouldn't say I was the bottom of the barrel, but I was certainly not their first choice for the position.*
>
> *It was an interesting environment at DNA. They hadn't had any recent leaks, but they had had some real encounters with the Containment Evaluation Panel. I remember one of their presentations to the Panel where they had decided not to present any of the mechanical closures, because they were only relevant to sample protection. Therefore, they refused to present any details about the closures because that was irrelevant to containment. Well, the Panel refused to categorize the shot, because they thought the closures were relevant to containment. Then Phil Opedahl, who was the Test Group Director on that event — I believe it was Husky Ace — stood up and said, "Just a minute. Mr. Chairman, could we have a short recess?"*
>
> *After the recess it was, "We'll provide you with any of the information you want. Whether these guys want to or not." And these guys were Jay Davis, and Lieutenant Colonel Davis — the Davis brothers. One was military, and one was civilian, and they weren't actually brothers — it was just what they were called. It was pretty clear where the concept that the mechanical closures were not containment features came from, because for many years thereafter Joe LaComb still insisted that the MAC's were not containment features. I never agreed with him on that point, and so we always described them fully in the CEP documents.*
>
> *One of the concerns was that it handicapped the Test Director to have all these non-containment features included in the containment presentation. But it was decided by the Panel that these features were important to containment. And, the Panel was right — you can say with confidence that after Mighty Oak those features were thought to be even more important. So, that's an old concept that's been abandoned, but I don't think DNA totally abandoned it until a couple of years ago.*
>
> *When I came to DNA their containment program was really being managed by S-Cubed. There was no Containment Scientist, and had not been one for over a year. DNA was doing as well as they could with the few military people they had, some of whom were quite new to the business. I wouldn't say they were*

desperate, but they were really being controlled by the contractors. DNA had very little in the way of technical capability in-house, so they really relied almost completely on their contractors, and S-Cubed was happy to step in and supply all the advice the DNA needed.

For me it was a totally new environment, dealing with contractors, because when I was at the Laboratory contractors were considered second class citizens; rude, mercenary, science-for-hire kind of people. They are still mentioned with a sneer. It was at DNA that I discovered that contractors did offer far more than you'd ever believe from the way they were considered at the Laboratory. I found they really were responsive, partly because you controlled the purse strings, but also because they were very capable. My whole staff was essentially contractors, and over the next ten years I gained a great deal of respect for them.

S-Cubed was the biggest one. After I got there, some people split off from S-Cubed and formed Pacific Technology. They approached me, and said they had some very good ideas as to what kinds of things were important in the groundshock closure of line-of-sight pipes, and the utility and the importance of the development of the residual stress near the cavity as a containment feature. I was also approached by Livermore people, who said they could do earth-motion calculations for me much better than anyone else.

So, I asked each of these groups to give me some examples of what they could do, and why they were good at it. That was sort of an informal shoot-out, and I selected Pac Tech as the people who gave me a detailed calculation, and showed me explicitly things that were really relevant to the program. At Pac Tech Dan Patch was the principal investigator, and they really did a good job for many years thereafter. They were very efficient — they could do large two-dimensional calculations very quickly. And, they had a very good grasp of the modeling, and the importance of those things to containment. So, Pac Tech was the second contractor. Physics International did some experiments for me later on, and Stanford Research Institute did a lot of experiments for me later on.

The whole concept of contracting for support does isolate you a bit from your staff, but you do define what the deliverable is to be, and what the price will be, and what the schedule will be. I found that I got results from the contractors much more predictably than, say, a program manager at Los Alamos would get from his staff. That's because his staff might be scattered all over the Laboratory, and he was always competing with other programs in the Laboratory for the attention of those people he needed.

I found I very much enjoyed the contracting process as a way of doing a program. It really worked, and we got a lot of good results. Though I was always worried when the contractors agreed with me. Was it because I had control of the money, or was it because they thought I was correct? And I was never sure.

The Laboratory or Agency which conducts a nuclear detonation is responsible for the selection of the site, and for the design of any features necessary for containment. The Manager of the DOE Nevada Office is responsible for the safe and proper conduct of the experiment, including the requirement that successful containment be accomplished. The Containment Evaluation Panel serves as an advisory body to the Manager, NVO. It is the responsibility of

the Chairman of the Panel to give due consideration to the judgments of the individual Panel members, summarize them, and make a recommendation to the Manager as to whether, from the point of view of the containment design, the event should proceed.

How well and how effectively the Panel has operated is, in some measure, reflected in the fact that there have been only four releases of radioactive material since June of 1971. For these four cases the total amount of material released was quite small — a total of some 10,000 curies — and was principally due to the seepage of noble gases from the cavity. A comparison of the post-CEP releases with a few of the major pre-CEP releases, and the total release into the atmosphere for the atmospheric detonations at the NTS is given in Table 1. Recall that for an atmospheric event the total fission fragment inventory is released. For underground events the release is always fractionated to some degree by the passage of the material through the earth, the tunnel, the pipe, or whatever the leak path was, and so the comparison numbers should be regarded with that reservation in mind.

TABLE 1

ALL POST-BANEBERRY RELEASES			SOME MAJOR PRE-CEP RELEASES		
Event	Date	Release (in Ci)	Event	Date	Release (in Ci)
Camphor	1971	220	Platt	1962	1,900,000
Diagonal Line	1971	6,800	Eel	1962	1,900,000
Riola	1980	3,100	Des Moines	1962	11,000,000
Agrini	1984	690	Baneberry	1970	6,700,000
	Total	10,810		Total	21,500,000

Release from NTS Atmospheric Tests 1951 - 1963 12,000,000,000 Ci

To the extent that the Panel has been successful, or deserving of some credit for the record of containment, that success is based on several things, the most important of which are these:

1. The Manager, NVO, and officials of DOE and its predecessor Agencies have been consistently and strongly committed to the need for successful containment of the events. They have also been consistently supportive of the Panel's activities and recommendations.

The CEP charter, in Section III - DOE Policies, Paragraph D, has the following words:

> **Considerations of cost, schedules, and test objectives shall not influence the containment review of any test.**

This charge is unusual in its breadth and in the authority it gives to the Panel. Since the formation of the Panel in 1971, every Manager, NVO, and every person who has headed DMA, or OMA, or DASMA, when asked, has emphasized that it was their intention that this charge be followed by the Panel. No member of any sponsoring organization has ever challenged it, to the Chairman's knowledge, or sought to modify or overturn a recommendation of the Panel. And, there have been occasions when the Panel's actions have caused considerable costs and schedule delays for a proposed event.

2. The Members and Alternate Members of the Panel do not serve as representatives of any organization. This is a critical point. They are individuals with experience in the field of underground testing, and knowledge relevant to the containment of underground detonations, who serve as independent experts and give their individual judgment concerning the containment aspects of an event.

   The Panel does not vote as to whether an event is expected to be successfully contained. The concern of a single member regarding some feature of a containment design has many times been demonstrated to be sufficient to require further review and resolution before the event can continue.

   It can be a difficult thing to convince skeptical critics of nuclear test work that the Panel is not some type of rubber-stamp group, staffed by the sponsoring organizations to give a public facade of responsibility for their activities. Individual integrity has unfortunately been so often shown to be lacking in governmental processes that to claim it for the Panel members is usually met with a raised eyebrow and clearly expressed doubt. Fortunately, the record of the Panel members' activities and actions has been sufficient to convince anyone willing to consider the evidence that the members do, indeed, seriously and honestly review the containment aspects of an event in the full spirit of the Charter.

3. The sponsoring organizations are the third and final part of having a successfully contained event. Here again, the matter of integrity and honesty is paramount. The Panel fundamentally takes the position that the material presented to them is, in fact, correct within the limits of the presenter's knowledge. A mistake may be made, but the assumption is that, if so, it is an honest mistake, and not a lie. A clear example is the number that is given for the maximum credible yield of the device. This is one of the most important factors in determining the scaled depth of burial, and the overall phenomenology of the event. That number as given is accepted by the Panel as the best that can be given for the particular device, and is never seriously questioned.

   In the same way, the Panel accepts as fact that the containment plan as reviewed by the CEP will be implemented in the field, and that the characteristics of the various containment features are as they are said to be. The seeps and the leaks

that can occur are really prevented by the people in the field who install the cable gas blocks, the cable fanouts, the stemming and plugs, and so on. The Panel relies on the integrity and competence of those people to do the job right.

In any organization or Panel that has operated for over twenty years, how it operates and how it might operate in a different manner is a question seriously to be considered.

Billy Hudson, alternate Panel member:

Hudson: *I think that by its very existence the Panel has a strong effect on the way testing is carried out. Knowing that you have to satisfy a Panel of relatively bright people who can ask penetrating questions causes you to look very carefully at your designs. It stimulates attention to detail.*

Carothers: I have sometimes thought that what it principally does is to put a person up in a public forum where some people are from, shall I say, competing organizations, and others are from their peer group throughout the containment community, and the CEP folks are going to ask questions. Most people have a certain amount of pride in a situation like that. Not that they're proud of being there, but they don't want to appear stupid in front of everybody.

Hudson: *That's right. That's part of it. Another part of it is they don't want to be caught doing something that appears to be stupid after the fact, if indeed there is a failure. So, the CEP is in many ways a public hearing before the fact, only to be brought to light should there be a problem. In that sense I think it has been a very valuable body.*

Carothers: What changes would you make in it?

Hudson: *It works. Why change it? You know it could be done cheaper, and you know it could be done faster, but you don't know it could be done better. If you said, "Well, gee. That's not a good enough answer. We really should try to do things as efficiently as we can, without sacrifice of quality," then I would say that we could probably make some changes in the CEP. I'm biased though. It's my opinion that phenomenology is the important thing to consider, in understanding containment, or affecting containment. Disciplines like geology, for example, are only supplying data for the phenomenologist to think about. In that context then, the role of a geologist, or a hydrologist, should be to say, "Yes, I think you have the right descriptive information," or "No, I don't think you have the right descriptive information." They shouldn't have an opinion about the containment of the event. I would say that in some ways you might have a more effective Panel if it were comprised basically of phenomenologists, and the geologists and hydrologists were cast in the same role as the drilling and cementing people. They would say, "Yes, we agree. You've got the right description," or, "No, we think there's a problem," but not make a statement per se, or categorize.*

Irv Williams, DASMA staff, DOE Washington

Williams: *One of the things the CEP has done is try to make sure that the Laboratory people have done their homework. And if they haven't, you know, it's embarrassing to be asked certain questions. I think the CEP is an absolute must, because with a venting, I think we would go out of business permanently. Another Baneberry, and I think we would be shut out of Nevada. And I don't know anyplace we could ever go back and test, without a furor, and that includes Amchitka. Therefore I think it behooves us to maintain the integrity and the questioning ability of the CEP to make sure the homework is done by the Laboratories, and that we feel relatively confident that we're not going to have a leak. Without that I think we jeopardize the future of any testing. And potentially the end of the weapons program.*

You do need to test, I'm convinced of that. I've been through too many experiments, and too many times we've had people who said, "It's a piece of cake." And then we get a surprise. Some are little, and some are big. We can generally stand the little ones. The big ones make you go back and do your homework. And you can't do it on a computer. You can't do it on a shot table at Site 300, or on a Fermex machine. The only way you can do that experiment is underground.

We have to have the confidence the CEP brings to the Directors (DASMA) here, because they do read the reports, and they do ask questions. Occasionally I have to come back and ask the Panel, "What did you mean?" The words are read, and it's amazing how well they are read by the Directors. The Directors, once they take the job, and they understand the responsibility that goes with it, want to make sure that things are complete, and we try always to make sure it is a complete package.

I've watched the Panel a long, long time, and I've attended meetings where there were some — inspiring discussions, let's say. I think you need to keep inquiring minds in there, and continue to realize that strange things do happen on shots. The people on the Panel need to realize that. That's the big thing, I think. They've got to realize that we get surprises out there. And I feel that maintaining our record is crucial.

Carothers: The CEP charter contains an unusual sentence, which says that in considering the containment design the Panel shall give no weight, pay no attention, to money, schedule, or data acquisition. That's an unusual charge that the Panel has.

Williams: *Yes, and it was intended at the time to say, "We know people will cut corners. We want to make this so corners aren't cut and there aren't incidents and accidents as a result of that." It was meant to give a strong hand to the Panel. That's also why they insisted on the independence of the Panel members.*

I have felt comfortable with the way the Panel has operated, and the fact that it remains inquisitive. I would encourage them to keep the Laboratory people on their toes in doing their work, because we all have a tendency to think we're old hands, and dismiss things. Try to make sure that the young bloods coming up are inquisitive, and very serious about their endeavors, so they really fully categorize the experiments. I think the life of the program, from the technical side, rests on our ability to assure containment.

Carothers: I think the people on the Panel, and in the Laboratories believe that too. But an attitude can develop in the Laboratories that the object of the CEP meeting is "to get this thing through." Rather than, "Let's go down and talk about it together, and see if there's something we missed." That worries me.

***Williams:** That worries me too. I think there should automatically be full disclosure to the Panel, because, what you might consider to be inconsequential, someone else can consider to be very serious. I feel that to be responsible they should have full disclosure, and do it in descriptive terms, so you can communicate with people back here, so we both understand it.*

To end this paper with some thoughts about the future, I wish to go once again to Bob Brownlee, who has thought about and concerned himself with this question of the future, and who made the following remarks a few years ago. I think a fortune cookie I had the other day contained the appropriate wise saying which could serve as a title to Bob's remarks.

There is no way of judging the future but by the past.

***Brownlee:** Most people probablly don't know that the Test Site was selected by Al Graves. He got on an airplane with somebody, they flew around; and he said, "Well, that's a nice area there. Let's just put some boundaries around it, and use that. There's a road to it on the south side, so we can get there. It looks like it's easy to build roads, and the security is such that we ought not to worry about the boundaries being penetrated at all."*

So, the criteria used for the selection of the Test Site had absolutely nothing to do with either atmospheric or underground shots. It was just a place we could get our hands on. It was a place that had a road to it, and a place where you could land airplanes; it was an accessible place. If in the fifties someone had said, "We want you to test underground. Pick anywhere in the United States you want and you can have it," we wouldn't have gotten a place as good as the Test Site. If we had used our heads we would have been in terrible trouble.

I think we were blessed, in a sense, by being put down in a place not of our choosing. We have had the best of things without having the freedom of choosing where in the world we'd like to go to do the best underground testing. As we have understood from the Soviets, and from the French, and from anybody else who's tried to test, we've had a wealth of different sites and different media, with opportunities for different kinds of tests. If we want to test in granite, we can; we don't have to go to North Africa. If we want to test below the water table, we can. If we want to test above the water table, we can. We can test in various kinds of alluvium, and in various kinds of tuff. Most places in the world are not blessed with all of those opportunities.

We were very slow to learn all of the different opportunities we had for the different kinds of tests that we wanted to do. For example, did we want to mine a big room? At the Test Site we could. Did we want to do something and throw a little dirt on it? We could. Did we want to lay something out on the surface and shoot it? We could. We could do that because in those very early years we were sufficiently removed from anybody that we essentially had everything around us, and a long way around us, under our control, with a few exceptions.

When we went underground the number of milk cows in the fallout pattern we might have would sometimes be three, or six. The number of people for whom you would have to provide the means of evacuation, were that to be needed, would be twelve, or twenty. What's happened with the Test Site since those days is that people have moved right to the boundaries of the Site, and now there are literally hundreds and thousands of everything and anything. But people see the Test Site as it is, and they don't understand that it was selected as it was.

Let me come back to underground shots. Early we had a hole and we used it. Even after Baneberry we still used the hole that was there. We had them stock-piled, and we tended to use them. It took us quite a little while after Baneberry before we really selected a site we wanted for the shot. I think it's only in relatively recent times that the containment people have had some input, really, as to which hole they wanted for a given shot. There are times when we will have a deep hole, and shoot near the top of it. That deep hole was drilled for something else, but we make use of it. I can find all kinds of examples, still, where we don't select the site as logically as we are able. We do things in tuff that might better be done in alluvium, and so forth.

As an example of what I am talking about, Los Alamos had at least three, and I don't mean that there might not have been four, shots in which we did a very low yield in saturated tuff. Now for us that's unusual, because low yields would normally be done in alluvium. These happened to be in saturated tuff. One time the guys came to me terribly excited because they'd had this low yield in saturated tuff, and they said, "When we drilled back, we hit the cavity, and the fans that do the ventilating were running backwards. All the air was going into the shaft; all of a sudden the cavity was just sucking air. How could that possibly be?" So I said, "The next time that happens, make sure you estimate how much air goes in." We had three shots for which we measured the flow of air into those cavities, and what we found, of course, was that the volume of air that went in was the volume of the cavity.

So, we had a standing cavity with a vacuum. What you immediately deduce is that the cavity was small, and in tuff, so it stood. It didn't fall in. But it was sealed off, and this told us a lot about gas flow through tuff, and how the thing could seal. It also told us about the condensation of the cavity material into liquids at the bottom. What I'm pointing out here is that we had the option, always, of doing a particular experiment under certain conditions which would allow us to learn things about containment, but we have done very little of that. It's tended to be by chance.

So, I would say that we have mostly muffed the opportunities to use, to the best advantage, the various shots that we've had at the Test Site. We have still learned, but it's not been nearly as much on purpose as it should have been. I'm afraid I would have to say that despite all the opportunities we've had to be clever, we've mostly just been routine.

We've had the opportunity to ask questions and to learn. And we've had a number of places where we could do this, and very little of that has been done. I won't touch upon the opportunities to learn about seismic things, but there have been opportunities to do that too, and I think we haven't done that very well. The Nevada Test Site is a remarkably wonderful place for us to have learned a lot, but we have tended not to do that.

I believe that as long as there's a nuclear stockpile we have to have the ability to address questions which may arise, and we have to have a place where we can go to answer them. It might mean a nuclear test, or it might not. Nowadays, I visualize that as being underground. Even chemical experiments could be underground, and not on the surface. We need a variety of ways to be ready to answer a variety of questions. So, we need alluvium, and granite, and tuff, and dry, and wet, and space, and mesas, and valleys. We need it all, because we do not know which part of it we can give up. I believe we will have a nuclear stockpile for at least the next four decades. Forty years. I can't conceive of getting rid of it in less than forty years time. I would like to, but I can't believe that we'll be able to do that.

They imported a lot of activities into the Site during the last moratorium. And when we go into another test moratorium, which I figure will come one of these days, I don't believe we'll ever go back to being able to use the Test Site again. If you look at what's happened at NTS in the last forty years, it's an exponential curve. With Baneberry we stopped testing, and at the end of six months, every week we delayed it was harder to start. We almost didn't get started up again; it got harder and harder as time went on. I think that if there is any kind of a moratorium in the future, it will be that way again.

And so, I almost despair over the loss of the Test Site, because I think it's happening, and NVO couldn't care less because they have no appreciation of what I've been trying to say. I've said what I think are the characteristics of the Site that are to our advantage, and I believe that there is hardly anybody in NVO who understands them. Most do not appreciate what it means to have such a Site.

I shouldn't say, "no appreciation" because I've been lecturing to them, and waving my arms at them, and writing things down and showing it to them. But it doesn't take. After all, we don't have to pay any attention to what Brownlee's saying. We've got this waste management program, and the President has said, "We've got to clean up. That's the urgent thing." So, NVO is no longer interested in stockpiles and testing; that's just way down on the list. In the memo we got from Watkins, where he outlined all the things DOE did, he never once mentioned nuclear tests at all. It's not listed in his long list of things that had to be looked at. Nuclear test is not mentioned. So, the DOE has little appreciation of what I'm saying.

And what's worse, neither do the Laboratories. This question about the Test Site I think is an exceedingly important question. We ought to recognize what the Site means to us, and I think we don't. Notice that I've said, "Despite all the opportunities we've had to use it cleverly, we haven't." And now I'm saying, "Even though we've learned as much as we have, we're getting ready to throw it away."

That's the reason why I spent quite a few months last year writing a document on the preservation of the Test Site. And I got Troy Wade to finally enunciate the DOE policy on the preservation of the Test Site, which in effect says to NVO and ALOO that any decision made about the day-to-day operation of the Test Site has to be made with the idea that we're going to preserve it for testing. I

was hoping by that means to get something down on paper which then I could wave in NVO's face, saying, "When you bring in these rug merchants, that is contrary to the policy of the preservation of the Test Site."

There is something kind of sacred about the Test Site nowadays. Put away the conservationists, and the preservationists, and the purists. We have done some things there that are special in history. They are. In the history of the world, a thousand years from now, that's going to be something kind of special. We ought to have that in mind as we act, but our concern is only this year's budget. That's all. And we ought to be bigger than that; we ought to be thinking more broadly than that.

I'm saying, "Here we have the Test Site. We could use it much more cleverly than we do, but we're only interested in the current budget with this fiscal year's shots. That's our only concern, and therefore, we just do things as they come." I think that's a grave mistake. I doubt that we will ever do it any other way because that's the way our government is, and that's the way we are. But I wish it were otherwise, and I would like to really understand more about containment by doing things of various kinds at the Test Site.

I really think that we have reduced the probabilities of venting so low that what we're apt to get caught up on is something trivial. That's what I think. I believe I'm more conservative than Gary Higgins is in some respects, but in other respects I'm more liberal. And Gary and I are convinced that nowadays the probability, by the time we get a shot reviewed and down hole, of it venting is very low for most of our shots. Of course, it's not the same for all shots, so when we do a certain kind of shot, the probability could be much higher. Now, I have argued you ought to react differently depending upon what the circumstances are. Gary takes the view, and I understand it, that it's good to always look for the worse case and plan your activities accordingly. My response to that is, "Yes, but that communicates the wrong idea to people." I spoke about that when we talked about Pike.

My feeling is that on the average shot now, if it cannot be compared to any previous failure, then we have to postulate something brand new to have it fail. And we have been testing long enough with a variety of different kinds of things that something brand new is highly improbable. Our luck has been that if it is likely to have happened, it would have happened to us. We would have given it the opportunity to happen already.

Now, that's true as long as you confine yourself to the Nevada Test Site. Notice what I said. "If you can't compare it with any failure we've had." That means at the Nevada Test Site. If I go to a brand new area, I now have nothing to compare to, so I have to assume, therefore, that I have to start over.

On Ledoux, Los Alamos is going to be putting an enormous amount of stemming in the shaft. "How much stemming do you really think we should put in there?" I said, "Well, I really don't think you need any. You know, to put stemming in that shaft is to spend the taxpayers' money for our own convenience, and it's wrong. It's morally wrong to spend all that money for nothing."

Sometimes we've not done things that we should. Sometimes we've not done them out of ignorance. Sometimes we've not done them because of the inconvenience. Sometimes we've not done them because of the money, but other times we have gone way too much in the other direction, and we have spent money that we really didn't need to just so there would be no criticism of us. I don't mind spending money that we don't need to in order to be conservative; that's all right. But I think it's wrong to spend money we don't need to just so there can't accrue any criticism of us. I don't think that's justified. You ought to take a little heat sometimes, for the sake of principle. This is a very difficult area, and I've seen us err in all directions, but it seems to me that there is a quality to all this which requires you to say, "Why am I doing these extra things? Am I trying to be conservative, or am I really just trying to avoid trouble for myself?" Sometimes we spend money, and it's a matter of convenience, and I don't think that's quite fair and right.

When you come right down to the pragmatics of getting something built and fired, there are lots of factors that are cranked in — convenience, cost, time, the personalities of the people you're trying to placate. Therefore, containment can never be a science. It's always going to be an art. You've always got to have these other things in there. You try to make containment pure and get it separated from personalities, but finally, they come in. You do have to remember there is a political component to containment.

We also have to avoid the event that will really cause great harm to the country, like shutting down the Test Program. It doesn't mean that the event itself is all that bad, but if, politically, it shuts down your program, then that's bad. And so, you do have to crank in the politics of the situation.

For example, Baneberry cost a lot more than money. Baneberry cost us twenty shots a year, forever. It cut the rate of testing in half. Why? Because we stood down for six months. And so, at the end of the year we'd only done half as many events as the year before, and everybody in Washington was so happy, because we'd saved money. From then on we have only had half a year's testing each year.

There is another point which I think is fair to talk about. I worry a little about the CEP when Jim Carothers, and Gary Higgins, and I are no longer there. I've learned not to trust some of those other guys, because they have not only no memory of the past, which is to be expected, but they really do not have the lessons of that history either. And therefore, they're capable of just going way off on crazy things, and there needs to be some old hands to balance things there.

We used to not have any turnover on the Panel, but we've had a lot of turnover in recent times. There are some people that you are just not going to educate, but there are a others who don't take the time to get educated. And in a while there's not going to be anybody to educate them. When I say that there can be human error, that we're apt to do something really dumb, one of the places where that can emerge is at the CEP.

I've done a thought experiment. Do I think that now, right now today, I could, on my own endeavor — although I'd like to consult Gary about it — design a shot in such a way that the probability of failure was enormously increased, but

I could still get it past the CEP without them catching it? Could I get all A's on it? There was a time when I would have thought, "No, I couldn't." And now I don't think I could either because of Jim, and Gary, and Carl Keller. But if I did just the right things, and conspired with the Chairman, and with Gary, I think I could put through something that would have a very much higher probability of failing than normal, and get straight A's on it.

And I also think, as Los Alalmos did with Ledoux, that we could present a shot that is a strictly straight A shot, and we could get all B's on it. We have demonstrated we can do that if we go about it right. Now, there's no harm in that, but there is harm in the first case. And I'll bet you that in five years the ease with which I could do my thought experiment will be greatly increased. And that worries me. Part of it is because the people only go back to '63, and as the years go by they don't even do that.

I have trouble with some of the geologists because they get so wrapped up in the geology that they think it means something. I think what I've learned is that the geology is only important when I'm on the edge. Then it becomes important. But if I've got normal margins of safety, the geology can be almost anything. I know that's true, because we've shot in almost any geology, safely. If you've done your containment design right, you don't have to hang on the geology to determine what happens. But if you've done things wrong, a trivial thing in the geology can make all the difference.

Now, don't misunderstand me — I just believe that we ought to be so conservative that geology never matters, and most of the time that's true. And so, I get very bored when they go into details that are of no importance to this shot; none whatsoever. But they go into it because after all, they've done this work, and they've got this geologic business to talk about. Well, they don't understand why it isn't important, and there's no way I can teach it to them. They have to learn it themselves.

Unfortunately we don't have the experience nowadays to show it to them. We test differently than we tested in '57 and '61 and '62. We don't have the same opportunities to learn by hands on. Therefore we have to invent a way of teaching it, and we haven't done that. Maybe you can't, but I fret over this aspect. So, these people, they're wonderful. They do their work conscientiously and carefully, but they haven't had the experiences we have had, and never will. Therefore they can never come at the problem in the same way. And I feel badly about that, but I don't know what you can do about it.

We'll never redo Pike, and if you haven't had Pike in your experience there are a lot of lessons you'll never learn. We're never going to do Pike again because we'll weed that out at the first glance. "No, we're not going to do that." That's the reason why I say that if you can't compare it with any failure, if it's utterly unlike any failure, it's not going to fail like any of those failures either. That means it's going to fail in some other way — like you didn't put in the stemming. Now, the probability of that is low, but it's not zero, and so, we have to be on the alert. But I'm afraid the young guys will do as they did with Ledoux, as an example. They should look at hydrofracing and they did. There was nothing else to look at, so they looked at hydrofracing until they convinced themselves it might hydrofrac. And then they convinced the Panel it might.

Well, good for them! They're all good people, and I'm not really criticizing them. I'm saying that this is the environment in which we live, and how can we live best in this environment? Well, we're not going to live as well in that environment when the old timers are gone.

Unfortunately, the Test Program has some aspects like that. You can't approach the Test Program with another Mataco, with another Pike, with another Des Moines. We're not going to do that, so those kinds of lessons will never be conveyed to a modern world. They'll have new lessons that they learn on other things, but will they be relevant? So I fret a little about containment.

DRAMATIS PERSONAE

I would like to thank the people whose remarks and comments make up the substantive part of this paper. In no particular order, they are Robert Campbell, Bob Brownlee, and Jack House, of Los Alamos. Cliff Olsen, Billy Hudson, Gerry Johnson, and Arnold Clark, of Livermore. Carl Keller and Joe LaComb, of DNA. Carter Broyles, of Sandia, and Irv Williams of the DASMA staff in Washington. It has been my privilege and pleasure to talk with these gentlemen at considerably greater length than there is here room to include. And for that privilege I should acknowledge the support of NVO, and LLNL, from whence the resources came to make those talks possible.

Talk is cheap, they say; transcripts of talks are not, but they are vitally necessary if the material is to be useful. For the transcripts my special thanks go to Beverly Babcock, who is skilled in the arcane arts of spelling and punctuation, and in the ability to transform spoken words to written words.

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MY THOUGHTS ABOUT CONTAINMENT

by

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ABSTRACT

An admittedly speculative overview of nuclear test containment philosophy and tunnel stemming predictive capability is presented. The main conclusions are that non-homogeneous rock motion produced by an underground explosion produces conditions where the residual stress field should not be expected to form with the magnitude, at the location, or at the time predicted by continuum models. Such non-homogeneous ground motions have been observed post-shot both along pre-existing fault or bedding planes and along planes not mapped pre-shot as unusual. Current pre-shot continuum analyses do not consider the possible impact of non-homogeneous ground motion on stemming plug formation. Recommendations for alternative calculational procedures, such as three-dimensional discrete element analyses based on measured block interface mechanical properties, are presented. It is recommended that, until these new analyses techniques are verified and shown to provide meaningful quantitative predictions, future test designs should continue to be developed primarily from empirical data and other experience.

1. INTRODUCTION

This report is full of speculation. It should be seen as an effort by the author to write down a number of thoughts and ideas which have been troubling him for many years. They should be read as concerns and as suggestions for additional research. This report does not represent the results of a particular technical investigation.

The author has had a long involvement with containment aspects of the underground nuclear testing program conducted by the United States. He sat on the Department of Energy Containment Evaluation Panel for almost 20 years, and he has been retired from regular employment since 1988. The first version of this document was written at that time.

Our underground test program began with the Rainier event fired in 1957. The test limitation treaty signed in 1963 decreed that all future tests should be conducted underground in such a way that no radioactive material would be allowed to cross our national boundaries. The continuing effort to attain and assure this objective is called the containment program. All testing organizations employ dedicated staff members whose primary responsibility is in the containment area.

The United States has developed two major testing configurations to meet the unique requirements imposed by the development of nuclear weapons and by the study of nuclear weapon effects. These are characterized most simply by the relationship between the ground surface, the mode of access, and the explosive device. Weapon development tests are usually conducted in vertical configurations which preclude personnel access to the vicinity of the device. On the other hand, effects tests usually occur in complex, horizontal tunnel arrays which do permit people to work close to the explosion point. Note that the word "usually" occurs in both descriptions. Exceptions to this simple classification sometimes occur.

The vertical tests are conducted in various geological settings and at various depths largely because there have been a large number with a wide yield range. On the other hand, the tunnel tests have been conducted in a limited number of geologically well characterized tunnel complexes. It follows that these differences, together with the additional requirements for sample protection, sample recovery, and underground recording on the DNA HLOS effects tests, have imposed different constraints on the types and extent of containment research that was conducted in association with the tests. As a result, more extensive and detailed work has been done in the tunnels. Fortunately, instrumentation could be installed relatively easily in complex geometric arrays in the tunnels and it was also sometimes possible to reenter the vicinity of a tunnel event to see what had happened.

In the early days of the underground test program, the testing community was impressed by the tremendous reduction in radioactivity release to the environment brought about by underground emplacement. Nevertheless, roughly 1/3 of all underground tests conducted before 1971 released some radioactive material. Sometimes the release was significant, but often it was exceedingly small. The data base lists some releases as small as 10^{-15}Ci.

In December of 1970, BANEBERRY was fired. This explosion vented a politically important amount of radioactivity, and it led directly to the tightening of containment requirements. After these requirements became effective, no prompt release was acceptable. Since then many changes and improvements in test design, in containment engineering, and in test evaluation have occurred. As a result, there have been only two small uncontrolled releases of radioactivity from DoE events in the last 20 years. The containment program has been a conspicuous success.

However, many of these changes were based on empirical observations and on good engineering design. We have not developed a thorough understanding of the immensely complex phenomena which accompany an underground explosion of a nuclear device. We know that very high pressures are generated and that they should last for a relatively long time. Why don't these pressures routinely push radioactivity to the surface? For a time, there was no credible scientific explanation.

Then in the early 1970's the residual stress concept was proposed, appreciated, and enthusiastically accepted by the containment community as the phenomenological explanation for our containment success. The concept is elegant and logical, it fills a strongly felt need, and it has been used, together with empirical observations, good engineering and previous experience, for test design and evaluation purposes ever since. To make quantitative predictions of these residual stresses, we have developed two-dimensional computer models based upon the classical continuum theory of material deformation.

A primary thesis of the present discussion is that these continuum models cannot provide quantitative predictions of material motions and residual stresses because they do not take into account the discontinuous "block" motions occurring along fractures and faults which are generally present in geologic emplacement media. As a result, these models are likely to overestimate the strength of residual stresses and possibly lead us to over confidence in containment evaluations. However, to some extent the containment community has long recognized that the two-dimensional continuum models are limited in that they cannot fully predict effects associated with the real world, three-dimensional in-situ structure or hydrofracture phenomena.

In support of this thesis, we will review the residual stress concept in the light of an admittedly biased selection of some present knowledge, explore the results of experimental efforts to confirm the presence of a residual stress field, and consider an alternative explanation for our containment success. A final section will evaluate the application of current calculational models to the problem of experiment protection in the horizontal testing configuration. Based on these observations and speculations, it is recommended that we make every effort to improve the realism of our calculational models by attempting to incorporate the potentially detrimental or beneficial effects of discontinuous block motions as well as the effects of other physical processes which may influence the calculational evaluation of containment prospects.

2. RESIDUAL STRESSES AND CONTAINMENT

Containment judgment has historically been based on empirical observations, good engineering and previous experience augmented by theoretical and numerical arguments where possible. Efforts were made to develop a containment theory in order to explain and understand the observed phenomenon that high pressure gases were retained within the cavity. The residual stress concept provided the first credible explanation.

The concept of a residual stress is one which is well established in considerations of the distortion of elastic-plastic materials, metals in particular. When, for instance, a simply-supported metal bar is subjected to a small uniform load, the bar deflects elastically. If the load is removed, the deflection vanishes. However, as the load increases, the tensile stress at the bottom surface and the

compressive stress at the top increase until the elastic limit is reached. Further load increases lead to plastic distortions which are not reversible. When the load is removed, the elastic strains try to relax. In so doing they develop compressive residual stresses in the material which had been plastically deformed in tension and tensile residual stresses where compressive plastic strain had occurred.

In spherical geometry, if a cavity is formed slowly by applying an internal pressure in an elastic-plastic material, plastic distortion will occur as shear strain. When the cavity pressure is reduced, the elastic stresses cause a material rebound, and compressive residual stresses develop in the region surrounding the cavity. The dynamically produced counterpart of these stresses are thought to be important to containment. With this introduction, consider the nuclear explosion case in more detail.

2.1 Phenomenology

Consider how we think about and calculate explosion phenomena. We know that when an explosion occurs underground, a shock wave is generated which propagates radially outward. The resulting ground motion forms a cavity and develops a plastic distortion field in the surrounding rock. We expect that the subsequent elastic rebound will recompress the plastically deformed rock thereby setting up a compressive residual stress field. If the strength of these residual stresses exceeds the cavity pressure, the gases produced by the explosion should be contained within the cavity. However, any departures from a simple uniform geometry and homogeneous material will introduce asymmetries which are the subject of much thought, study, calculation and conjecture.

We have developed sophisticated calculational tools to describe the explosion process in detail. These tools are based on the well known Lagrangian or Eulerian continuum mechanics formalisms. It is recognized that these procedures require an accurate description of the response of geologic materials if they are to provide reliable results. As a result all testing organizations have developed sources that attempt to supply the needed material properties data. The several testing laboratories have also devoted effort to the development of constitutive and equation of state models which can incorporate experimentally determined material properties and output material response details demanded by the codes. Most of these models are built upon the foundation of the elastic-plastic formalism.

These calculational models are routinely used in their one- and two-dimensional manifestations to; 1) aid in the design of ground motion diagnostic experiments, 2) estimate ground shock environments, and 3) help evaluate prospects for containment. This broad application and ongoing evolution and calibration of the models has led to a general acceptance of their qualitative predictions even though there is wide spread knowledge of their limitations and potential pitfalls. The limitations are the result of material property uncertainties or the difficulty in defining the strength of "damaged" materials as pointed out

recently by Proffer and Rimer, (1990). Other limitations may come from our recognized inability to fully characterize the specific features of a particular geologic emplacement medium, our use of two-dimensional codes to describe a three dimensional world, and the inability of the present models to include dynamic mixing processes or to consider the potential for dynamic hydrofracture.

Although the testing community has used these calculational models for a number of years, their experimental validation is largely based upon laboratory experiments and field tests which are much smaller in scale than nuclear events. Moreover, the existing data for nuclear events has been largely acquired by DNA in their horizontal tunnel configurations mainly because it is more expensive and difficult to make definitive measurements in the vertical configurations.

2.2 Experimental Measurements

Laboratory work has been successful in demonstrating the formation and decay of a residual stress field. In particular, a series of experiments was done in very carefully prepared, 30 cm diameter, grout spheres containing small explosive charges at SRI, (Cizek and Florence, 1983). The pressure required to hydrofracture a high explosive cavity was measured as a function of time after the explosion. The data clearly showed that a residual stress was generated, but it also showed that the stress rapidly decayed. These tests included particle velocity measurements as a function of time and radius after an explosion. The data showed the expected elastic rebound at late time, and the quality and range of the data allowed it to be used in the development of an improved material response model, (Rimer, Lie and Cherry).

It is significant that the distortion of the grout spheres was qualitatively consistent with one-dimensional calculational predictions. Specifically, the grout appeared to have moved as was predicted for a homogeneous isotropic material.

Similar results have been obtained in a series of high explosive tests conducted in the tuffs of Area 12, NTS. Again the scale was small compared with that of naturally occurring geologic inhomogeneities, so the associated asymmetries of the motion would presumably be less significant than those occurring on the much larger scales of interest in nuclear testing.

Field measurements on nuclear explosions are much harder to make largely because it is difficult to assure long-term instrumentation survival. Several classes of measurements have been made. The simplest having some value in verifying residual stress predictions is the measurement of peak values of ground shock stress and particle velocity. Such measurements have also been made on vertical shots. These quantities do agree with calculational predictions, but it is well known that such predictions are not sensitive to some of the material models, and they do not provide reliable indications of the late-time rebound motions or the associated residual stresses. For this reason it has been possible to develop empirical relations

which apply reasonably well to large bodies of this experimental data without requiring site-specific input, (Perret and Bass, 1975).

More subtle calculational predictions have usually not been verified because we haven't been able to directly measure the entire history of the stress and velocity pulses or the characteristics of the residual stress field. The difficulty of these measurements under nuclear test conditions has also precluded detailed comparisons of measured and predicted displacements.

A major problem is that instrument cables are often broken by non-uniform ground motion before residual stress data can be obtained. Nevertheless, Sandia has obtained a number of records which may indicate the presence of a rebound which should lead to a residual stress.

An effort was in part made by TerraTek to circumvent this problem by developing a system for measuring the pressure required to hydrofracture the Area 12 tuff immediately after an explosion. This system was triggered by ground shock, and (it) required(s) no electrical connection to the outside world. No definitive data have yet been obtained, and the data so far available suggest that the expected residual stress field was either not developed or that it decayed very rapidly.

A qualitatively similar approach is being pursued by Sandia. They have developed Self Contained Environmental Monitoring Systems (SCEMS) which contain conventional stress and acceleration instruments in very strong vessels which can survive ground shock stress and displacement. The instruments are battery powered, and the data obtained is stored in on-board memory. So far little data has been recovered from this system, (Bass, 1990).

Note that both the TerraTek and Sandia techniques requires post-shot reentry mining to recover the data. This is expensive, and it is becoming operationally more difficult for environmental health and safety reasons.

It seems fair to conclude that our efforts to directly measure the residual hoop stress field directly in a nuclear test have not yet fully succeeded. We have, however, obtained a great deal of experimental information some of which guides the development and verification of quantitative models and some of which raises questions about the models.

2.3 Reentry Observations

Over the years a great deal of evidence has been gathered which shows beyond the shadow of a doubt that the displacement field produced by an underground nuclear explosion at NTS is not uniform on a dscale small compared with the cavity radius. It is not one-dimensional within, say, two cavity radii of the explosion. This data has come from reentry observations made in tunnel complexes.

Nonuniform ground displacements were originally observed and were carefully documented on the first underground test, Rainier. An extensive reentry program was conducted after that event to investigate explosion phenomenology. In particular, a tunnel was driven around the lower hemisphere of the explosion cavity. The following paragraphs are quoted from the conclusion section of the report of this work, (Wadman and Richards, 1961).

> "A zone of plastically deformed rock developed in front of the rapidly growing cavity after detonation. This zone is still intact around the lower cavity limit and varies in thickness from 3 ft. to 4 ft. Fused material was injected into the plastic zone along tension fractures, which developed at rather regular intervals along the bottom edge of the cavity. The tension fractures seldom exceed 2 ft. in length.
>
> "Rock that once occupied the space within the cavity moved radially outward in all directions from the point of detonation. Blocks moved by minor adjustments along the many shot produced shear planes developed in the competent rock beyond the plastic zone. Much of this movement was resolved along pre-existing zones of weakness, such as bedding planes, faults, and joints ..."

Many complementary observations have been made over the years during reentry investigations following DNA events. Two examples from a large population include work on MIDNIGHT ZEPHYR/TOMME and MISTY RAIN. In the former case, a hole, RE#2, was drilled toward the TOMME WP through rock which "should have been" simply displaced tuff. A wide variety of things were found in the hole including grout, metal, rubber, and bits of cable which must have been transported to their final resting places by decidedly non-radial displacements, as described in the TOMME/MIDNIGHT ZEPHYR Containment Summary Report.

The reentry following MISTY RAIN identified at least nine faults which were generated by the explosion. See Townsend and Baldwin, to be published. These faults were within a cavity radius of the cavity edge. Bedding plane slip was not seen, but severe distortion - folding actually on a scale of less than 1 meter - was observed in at least one place. This ground motion has at times been described by saying that one just can't understand shot-induced displacements close to the cavity. They are chaotic and blocky.

2.4 Implications

Although we have long known that the displacement field departed significantly from the idealized, quasi-radial motion predicted, we have not explored the possible implications of this departure. Since the residual stress field is driven by the displacement field, there should be some implications even though calculations made with modern material models predict that the peak of the

residual stress field lies outside the region of pronounced chaotic and blocky motion.

The residual stress field is the product of an interaction between elastic and plastic strains. Thus, if the displacement and strain fields are significantly affected by discrete displacements of joints before and after rebound, the residual stress will also be altered.

At early times the high ground stresses generated by the explosion will cause plastic strain in the material processed by the shock and the overall flow field will be influenced by the way materials respond on distance scales up to cavity size. If distortion can occur on discrete interfaces, the character of the plastic deformation will be changed.

At late time there will be an elastic relaxation, and a residual stress will be generated. The magnitude will depend on local conditions which, in turn, depend on whether or not discrete motion occurs nearby and what "strength" characterizes that motion during rebound.

This does not imply that there will be no residual stress field, but rather that we cannot rely upon conventional continuum mechanics models to correctly predict the magnitude and location of these stresses. Furthermore, if the ground motion is inhomogeneous, with displacements localized along planes of weakness, it may be intrinsically impossible to predict the details of the motions and the associated residual stresses. Finally, it is almost certain the residual stresses will be degraded by these localizations and asymmetries of the motion and by the reduction in bulk strength of the near field rock at the time of rebound.

If these assertions are correct, there is every reason to expect that, in some cases at least, the residual stresses have been significantly weaker than predicted by continuum calculations. Yet even the worst of these events (with the notable exception of BANEBERRY) were safely contained, apparently by some mechanism other than a strong fully-encompassing residual stress field. Before suggesting a candidate for this alternative mechanism, we will review some of the relevant data.

3. VERTICAL SHOT CONFIGURATIONS

The preceding discussion has relied on observations from horizontal test complexes which facilitate instrumentation and reentry mining. In addition, there are two observations related to vertical tests which support the above arguments. One deals with cavity size observations. Samples are routinely obtained for radiochemical measurements by drilling into the glass below the WP. The lower cavity radius can be determined from measurements of radioactive intensity and hole location. Such measurements have been made on hundreds of tests. The materials have ranged from wet and dry weak tuff to wet and dry strong fractured lava. Considering the approximate nature of these measurements and their degree

of scatter, they should not be viewed as accurate determinations of the cavity sizes produced by individual events. Nevertheless, one would still expect to see a statistical relationship between these data and the properties of the emplacement media (*e.g.*. strength) which control calculational predictions of cavity size.

A statistical analysis done years ago indicated that the measured cavity size was roughly independent of the test medium. The observed variability between different media was not large compared to the experimental uncertainty. This is in contrast with calculational predictions which suggest a much greater dependence on the rock strength. A possible explanation for this discrepancy is that the bulk strength of the rock mass is controlled by the sliding characteristics of joints and fractures and that these properties are relatively insensitive to the strength of the rock matrix. Of course this argument is not conclusive since different rock masses may have different joint and fracture characteristics.

Some support for this view comes from calculational efforts to explain the ground motion measurements made on the Piledriver event which was conducted in jointed granite. The Piledriver cavity was smaller than events in other rock types, however, efforts to match the observed cavity size required that the bulk material strength be severely degraded from that of core samples. Equally severe reductions of rock strength are needed to explain the cavity sizes seen in tuff and alluvium (Day, Rimer and Cherry, 1982 and 1983), though the conventional explanation is that this strength reduction is caused by micromechanical damage which can be measured in postshot cores (Proffer and Rimer, 1990). Observations indicate that bulk strength reduction does occur during cavity formation, that the rock matrix is damaged micromechanically, and that the joints do slide. The relationship among these three processes remains an open question and may even be unique from event to event.

In this context it should be reemphasized that our most reliable and consistent measurements, those of peak radial stress and velocity, are not very sensitive to the strength reduction process, whatever its origin. Hence, the ability to predict these measurements is not taken as a verification of calculational models. Conversely, the characteristics which are most sensitive to strength reduction (*i.e.* peak displacements, residual stresses, etc.) appear to show much more variability. Although this variability is partly a consequence of measurement difficulties, it may also be indicative of the inherent asymmetry and lack of repeatability associated with a highly nonuniform displacement process.

A second major observation regarding vertical emplacements is based on calculational studies of LLNL events. These analyses suggest that on several occasions nuclear tests have been emplaced in geologic configurations where the calculated residual stress was marginal but in which the other containment features were satisfactory. Two examples are COTTAGE and BARNWELL. Both shots were conducted as designed, and they were contained according to the CEP definition. If the residual stress calculations are qualitatively correct then the BARNWELL and

COTTAGE containment performances suggest that at least in these cases strong residual stresses apparently were not necessary for early time containment of high pressure gasses.

Although contained at early times, BARNWELL did produce a late-time seep of radioactive gases which was seen within a week after detonation during a period of falling barometric pressure. Similar late-time seepage has occurred on a number of other LLNL events and is generally associated with the presence of vertical cooling cracks in the welded tuffs above the cavity on certain PAIUTE MESA events. However, the upward transport of cavity gas may have been aided by the growth of dynamic hydrofractures which are expected to occur when residual stresses are too weak to contain the cavity pressure. This explanation is supported by Rimer's dynamic hydrofracture analysis (Rimer, 1990) which predicted that gas-driven fractures might penetrate halfway through the overburden, reaching the geologically fractured tuff through which seepage to the surface may occur. The important point is, however, that the event was still adequately contained in spite of the predicted weakness, and possible failure of the residual stress field.

In addition to these site-specific calculations, there have been a number of more generic studies which also suggest that residual stresses may often be too weak to ensure containment. Several years ago Swift, Rambo, and Bryan presented a paper at the Third Symposium on Containment of Underground Explosions, Swift, Rambo and Bryan, 1985, in which they looked at the influence of complex axisymmetrical geometries on the residual stress field. They found several conditions which could significantly weaken the field and raise questions concerning containment prospects. They conclude in part:

> "...For complex sites, numerical simulations are instructive in examining phenomena, but may not provide decision making information for assessing containment reliability. In this context, effort should be made to develop additional calculational criteria for the successful containment other than having to rely on the concept of a strong, sufficiently thick residual stress field ..."

As previously stated, we have by design successfully conducted tests at sites where our continuum calculations show that we would not expect a strong residual stress field to form. By extrapolation one can also imagine that over the years we may have fired other devices in geologic situations which would not have led to confident residual stress predictions if we had known the geologic reality and/or if we had made the calculations.

Rambo extended this work in a paper read at the Fourth Symposium, (Rambo, 1987). He showed a number of geological situations where calculated residual stress is below cavity pressure. He also summarized the work done to explain the BANEBERRY failure. He showed that a good residual stress should not have been expected in an axisymmetric representation of the BANE-BERRY

geometry. In addition, a region of "tensile damage (void strain)" developed from the cavity to the surface. Presumably the vent followed this underdense path. A troublesome feature of this analysis involves the timing of the vent. If a more or less open path existed between the cavity and the surface, why did it take over three minutes for very hot, radioactive material to escape? Perhaps there have been events in which slow moving intrusions of gas broke out of the cavity but never reached the surface.

3.1 Alternative Containment Concept

The preceding paragraphs have presented several arguments which suggest that the residual stress concept, which has been important to our thinking for over 15 years, may not provide a definitive scientific explanation for the containment of nuclear explosions. Strong residual stresses, if they could be produced, may be sufficient to achieve containment, but they are probably not necessary. Is there an alternative containment mechanism? Perhaps there is.

Griffiths and Nilson, Griffiths and Nilson, 1985, described a containment concept based on gas-driven fractures at the Third Symposium. They showed that in the complete absence of a residual stress field and heat transfer within the cavity, penny-shaped fractures would grow only part way to the surface before gas diffusion and heat transfer losses stopped further growth. Specifically, if they limited fracture growth to half way to the surface, they derived a containment prescription which looks remarkably like the depth of burial criterion now used in siting our tests. Furthermore, they found the present criterion to be extra conservative for large yields - a situation which has been apparent empirically for a long time. This conservatism might increase if fracture length as a fraction of burial depth could be replaced in the theory by a criterion which required a particular unfractured distance from the surface. This would permit relatively longer fractures for large yield events.

Without question, insufficient work has been done on this concept to justify its acceptance. However, it appears to show great promise. The penny-shaped fracture assumption, which is clearly unrealistic, has subsequently been relaxed through the work of Nilson and Morrison, also presented at the Third Symposium, Nilson and Morrison, 1985, which shows that an "envelope-fracture" growing only toward the surface would propagate no further than the simpler fracture. This is because a more narrowly directed fracture has smaller opening displacements, a smaller propagation velocity and, hence, greater losses of heat and mass. Limited work has been done on the influence of material properties in affecting opening displacements. Much more needs to be done.

Several comments are in order:

1. There is some experimental data to support a fracture-based containment concept. In the early days of the underground test program, radiochemical drilling was done from the subsidence crater. Radioactivity was often observed

almost half way to the surface. This may have been related to fracture growth in the upward direction, and it may simply be the result of diffusion from the chimney, but some of the data suggest that gas intrusion occurred before chimney collapse.

2. There is good reason to believe that cavity pressure was successfully measured on Cornucopia by Hudson et al, Hudson, et al., 1989. The measured value dropped to the minimum *in situ* stress within less than a minute, suggesting no residual stress was present to prevent cavity gas from escaping from the cavity along some fracture system.

3. RED HOT was not successfully contained, however there is good reason to believe the leak would have been much worse if multiple short fractures had not grown from the cavity at early time. Arguments have been made that the leak was the result of a poor containment system design. It would not have happened if modern stemming practices had been used. Cavity expansion was less than half that calculated by conventional techniques. These calculations showed the observed expansion could happen only if the cavity pressure were dramatically lowered in about 10 msec. Other calculations suggested that such a reduction could be caused by a system of fractures about 3 m apart. (Nilson, 1985). One such fracture was observed in the reentry drift. It was only about 5 m long. Fourteen others were found in a drill hole along the flat face of the cavity at an average spacing of 3 m. Two long hydrofractures, that extended from tunnel level to the base of the vitriz tuff–40 m in length, were also observed above the PEEP WELL access drift. I suspect these fractures occurred at late time as compared to the system of fractures that extend from the RED HOT cavity.

4. There might be some comfort in a theory which based containment prospects on more easily measured material parameters than is presently the case. The main unknown in the fracture theory is the rock permeability. This is a quantity which can, in principle, be measured *in situ* over a large averaging interval from bore holes. Several LLNL measurements of subsurface pressure response to changes in barometric pressure (Burkhard, Hearst and Hanson, 1987) indicate that the bulk permeability of volcanic emplacement media is typically 10-100 Darcies, which is comparable to alluvium and is probably sufficient to ensure containment.

5. There might be additional comfort in a theory which did not rely on knowledge of close-in material motions. The fractures are predicted to propagate well beyond the chaotic region discussed above. Also, containment appears assured by gas flow into fracture surfaces. The specific location of a fracture or fracture system is not important. Only the existence of the fracture is.

6. Since the fracture propagation rate appears to be relatively slow, a fracture-based description of explosion phenomenology might provide a credible explanation for the relatively late time of those few vents which have occurred. Even the

primitive theory presented by Griffith and Nilson showed that fractures took several seconds to reach half way to the surface. Nilson and Morrison showed that the time required for flow to reach the surface through a fracture could easily be measured in minutes.

7. The containment failure at BANEBERRY may have been the consequence of two unfavorable conditions, a weak residual stress field and the presence of a significant fault which served as a vertical pathway for the flow of gas. It is quite possible that the faces of the BANEBERRY fault are sealed by clay or gouge, preventing the lateral seepage which normally impedes the upward flow of gas. To test this hypothesis or guard against future occurrences, conceptually one could perform measurements of permeability across a questionable fault.

8. One might well ask why more radioactive fractures have not been seen in tunnel reentries if fracture propagation were important to containment. Of course the question needs to be carefully investigated, but a provisional explanation would suggest that the main fractures are envelope-shaped, and they run predominantly in the vertical direction. That's not where our reentry tunnels go, and the evidence of fracture propagation would usually be obliterated by collapse of the chimney. Evidence for a vertical fracture was found in the Madison reentry.

Little effort has yet been made to develop a sophisticated fracture-based containment theory. Even less has been done to critically evaluate it. With the exception of RED HOT no effort has been made to seek experimental verification of such a theory. Our community may be pleasantly surprised if, and when, such efforts are made.

3.2 Summary

A number of arguments have been presented which question the validity of the residual stress concept in terms of providing the scientific basis for understanding the successful containment of underground nuclear explosions. The main point is that extensive reentry data shows that the ground motion near an explosion is not essentially one-dimensional over distances larger than many containment features as assumed in our calculational models. Therefore, these models cannot quantitatively calculate how the rebound may recompress the irreversibly distorted region surrounding the cavity. As a result the actual residual stress field would not be expected to possess the predicted strength, at the predicted location, or at the predicted time.

The author hopes others will critically examine these arguments and reevaluate our current understanding and calculation models of the residual stress concept. Two avenues of investigation appear promising.

First, we should attempt to improve the realism of our ground motion calculations by developing computational models which incorporate the effects of inhomogeneous displacements caused by geologic discontinuities and by strain localization processes. Progress could be made in this direction by application and adaptation of existing "distinct element" models, as further explained in the recommendations of Section 4. Such models will allow us to qualitatively downgrade our expectations of residual stresses, from those predicted for homogeneous motion.

Second, we should explore alternative containment mechanisms capable of containing gases when residual stresses are expected to be small or they cannot be reliably calculated. One such alternative is the fracture-based containment criterion proposed by Griffiths and Nilson. In some respects, this approach is more conservative and may provide a scientific explanation of why large yield explosions have always been contained. It is recommended that this theory be developed as soon as possible, then be critically evaluated to define its sensitivities, strengths and weaknesses, and finally experimentally verified or rejected.

4. HORIZONTAL TUNNEL TESTS

The previous discussion argued that continuum models cannot provide quantitative predictions of residual stresses. In this section that discussion will be extended to the stemming column emplaced around lines of sight used in weapons effects experiments. It will be argued that residual stress and stemming behavior are both strongly influenced by block motions which are not included in current predictive models. First, a reminder followed by a brief introduction. The reminder is that this paper presents the speculations of an author who retired from active work over five years ago. He has not been intimately involved in DNA tests since MISTY RAIN.

4.1 Sample Protection and Stemming Design

A large fraction of weapon effects tests are conducted in horizontal tunnels. Most of them make use of a line-of-sight pipe to conduct nuclear radiations from an explosion to a test chamber containing various items to be investigated. Diagnostic instrumentation is usually placed close to the test chamber. It is the job of the containment community to design and emplace tunnel stemming and closure hardware to reliably close the line-of-sight after an explosion and prevent the release of energy and radioactive material from the vicinity of the explosion.

DNA has developed a conservative containment design which supplements this close-in system with two additional barriers to prevent any prompt radioactive release to the environment even if the first should fail. This "Three Nested Containment Vessel" design has proven very effective. There has been no containment failure associated with a DNA test in about 20 years.

This last statement needs a bit of explanation. Officially, successful containment is defined as "containment such that a test results in no radioactivity detectable offsite as measured by normal monitoring equipment and no unanticipated release of radioactivity onsite within a 24-hour period following execution, DOE, 1992" and this requirement has been met. However, on a number of recent events there has been leakage through the first containment barrier into portions of the tunnel complex. In two extreme cases, this has caused contamination of experimental samples and diagnostic systems with the associated loss of data and equipment *i.e.*, there has been failure of sample protection. In these instances, when the tunnel complex is reentered and ventilated, a small amount of inert radioactive gas has been released to the atmosphere under carefully controlled conditions.

Effort has been spent designing improved stemming plans and attempting to understand the radioactive seepage which sometimes occurs from the cavity to the tunnel complex. One analytical tool has been the computer and I believe the arguments made previously concerning applicability of continuum models apply with increased force to this effort.

The stemming region includes the interval from the cavity boundary to roughly two cavity radii outside the cavity. On some events, observed ground and stemming motions in this region, do not agree with those predicted by our calculations. There is data showing that block motions can be a prominent component of the overall displacement field in the horizontal plane of the tunnel complex as observed on reentry. In those cases the residual stress values, locations, and histories would be expected to differ from those predicted by continuum models.

At best we expect the current two-dimensional continuum calculations to apply to some average, idealized configuration. Unfortunately, we work in real three-dimensional geologic media containing fractures and faults that may cause significant deviations from the idealized continuum response. For this reason we cannot know that our calculations will be conservative. As a result, tests requiring containment designs that significantly depart from previous experience have been preceded by development/proof tests. Examples include the DIAMOND ACE, MIDNIGHT ZEPHYR, and DIAMOND BEECH, tests that preceded MIDDLE NOTE and the MISSION GHOST test that preceded MISTY ECHO. Proof tests have also been performed, as required, on closure hardware prior to their inclusion as a containment feature.

Ground and stemming motions cannot be quantitatively predicted with current codes that model explosively driven ground motion. These codes tend to suffer from two primary shortcomings, (1) the omission of discrete material interfaces and planes of strain localization and (2) the impracticality of numerically resolving the relatively small length scales involved in turbulent mixing of severely shocked materials. After further explaining the nature of these phenomena and their importance to LOS closure processes, some recommendations will be

made regarding what this author believes are promising pathways toward improved computational modeling.

4.2 Observations on LOS Closure

The calculations that have been done for test design and containment evaluation assume that the test medium is homogeneous and uniform. The codes are based on continuum mechanics formulations which assume that initially adjacent mass points will preserve their relative relationships as a distortion occurs. Observation of fracture, fault, and bedding plane displacements tell us this isn't always true. The evidence suggests that the test media are essentially non-homogeneous on a small scale compared with an explosion cavity. Their distortion and displacement are not always controlled only by ordinary plastic distortion and material strength as currently measured. Rather these discrete motions depend, to a large extent, on the presently unquantified properties of interfaces between adjacent blocks or mass elements.

These processes, which are not presently included in our theoretical models, may help to explain the apparent degradation of test containment performance as summarized by Peterson, (1989). Figure 4-1 taken from his paper illustrates the observations following 14 "standard yield line-of sight-tests". The recent experience is the source of much concern. Of course, there is no unequivocal answer, but the author believes a general explanation is available. It will be presented after specific comments are made concerning MISTY RAIN.

A few of the close-in MISTY RAIN observations include:

1. Grout was apparently injected significant distances into the LOS opening in the stemming before the arrival of ground shock (the pipe itself was, as is typical, destroyed). Stemming calculations do not predict the observed grout extrusion.

2. Radioactivity seems widely and more-or-less uniformly distributed throughout the stemming grouts with noticeable concentrations within the grout filled LOS opening and near the top of the original LOS drift.

3. There is ample evidence of "turbulent" rather than smooth, continuous flow of stemming materials.

4. Post-test tunnel cross-sectional areas were measured over a limited range. From RS 2+30 through RS 3+02 the post-test tunnel cross-sectional area was smaller than the preshot value while the RS 1+88 and 2+02 areas were larger than the preshot values. These smaller areas are, I believe, unique to MISTY RAIN since comparable measurements on previous events indicated greater tunnel expansion.

A plausible, but by no means proven possibility is sketched in Figure 4-2. It is hypothesized by the author that discrete, non-uniform ground motion was a

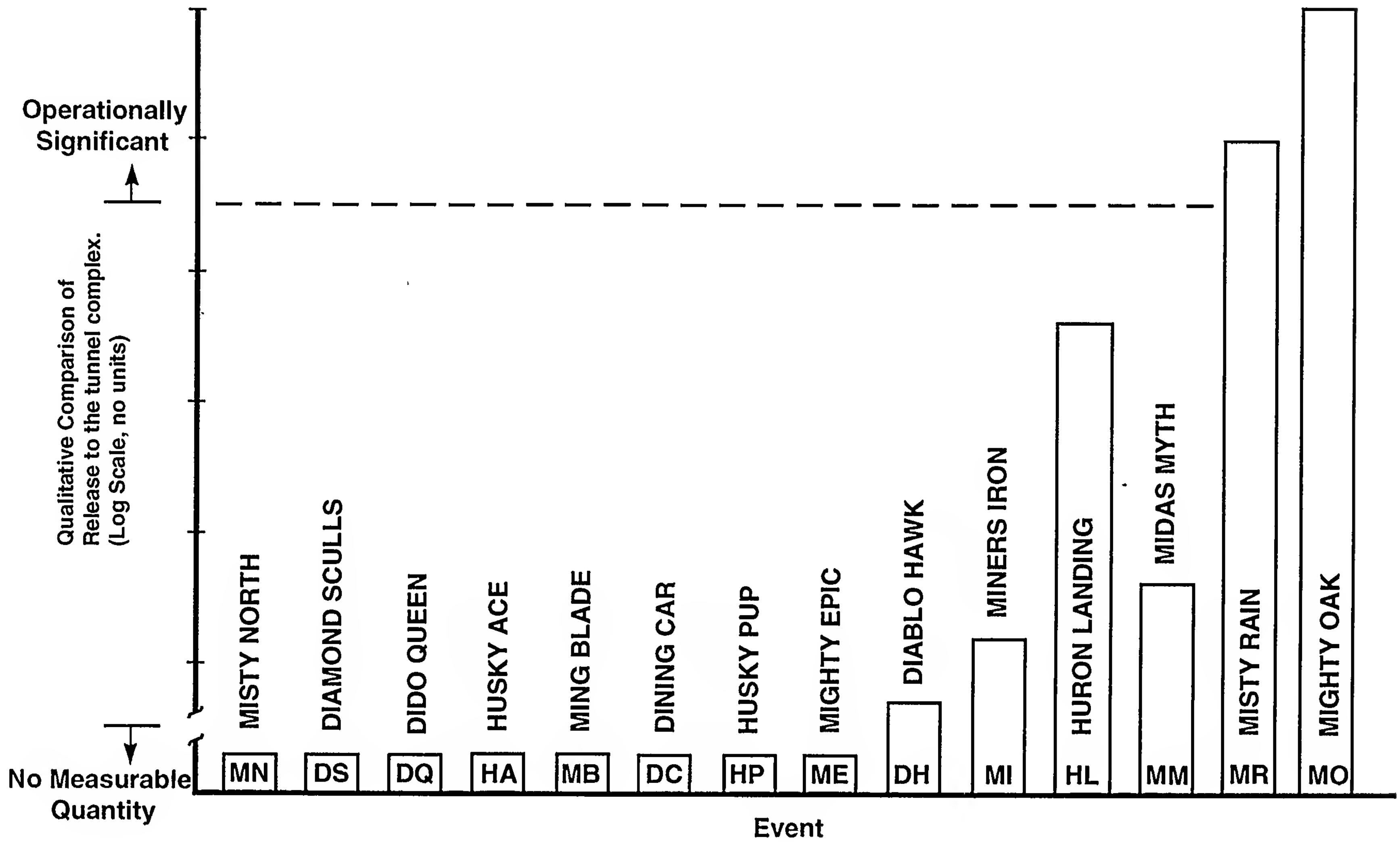

Figure 4-1. Qualitative comparison of radioactive gas leakage into the tunnel complex for events MISTY NORTH through MIGHTY OAK.

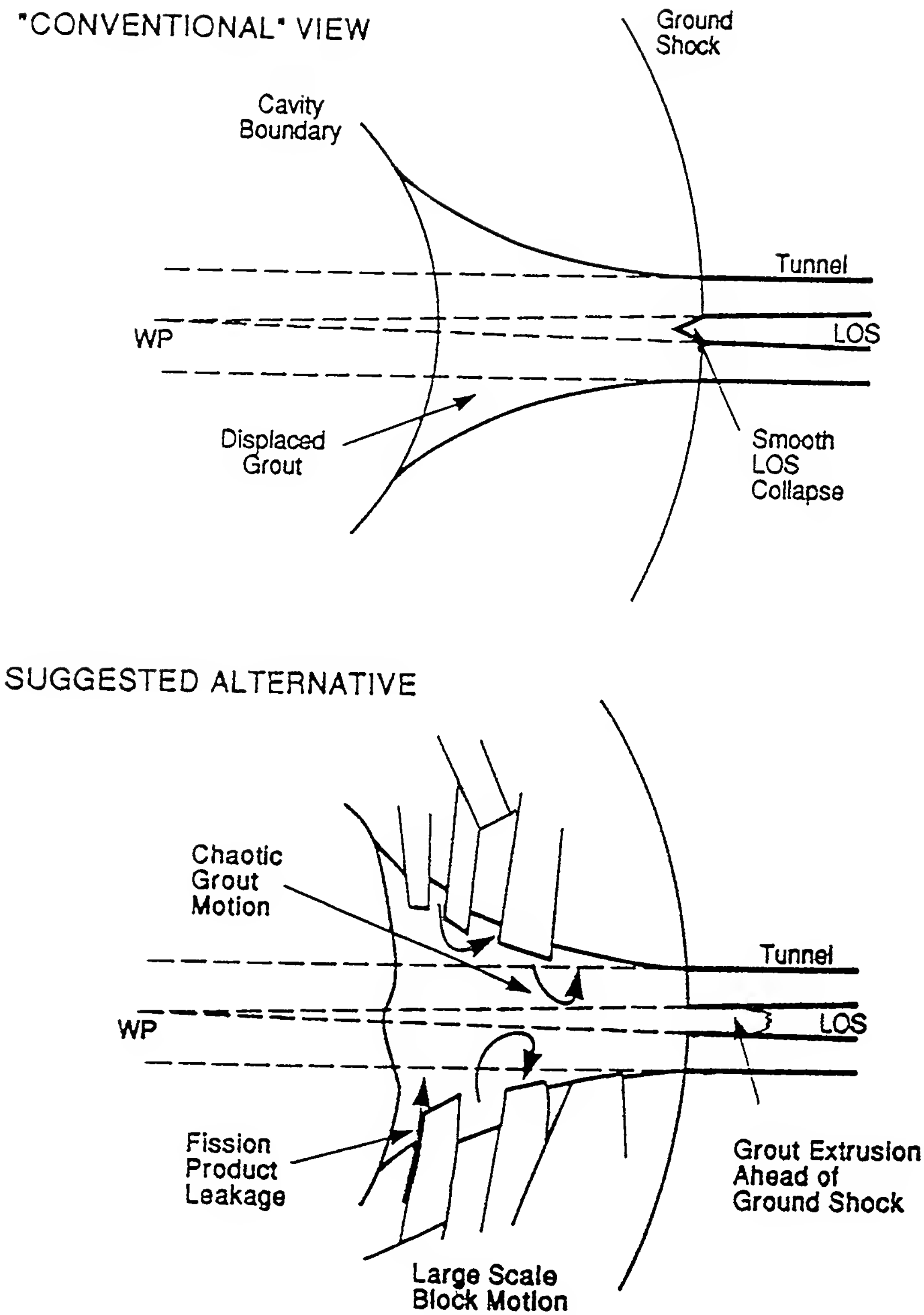

Figure 4-2. Conventional and alternative views of stemming motion.

significant component of rock displacement near the forming cavity. For some reason unique to MISTY RAIN, this motion led to less net tunnel expansion near the cavity than predicted by conventional calculations using relatively weak rock material properties. (For other events there may have been more tunnel expansion than predicted.) As a result, grout extrusion was excessive, and a plug of grout was forced into the LOS ahead of ground shock. This may explain the observation that grout stagnated against the MAC housing before ground shock arrival as measured by Bass, (1987). The discovery of part of the reverse cone near the original muffler location and the large displacement of passive gages is consistent with this picture. The leading edge of this grout plug may have squirted ahead and destroyed the MAC before it closed completely. The fast moving grout plug may have acted as a piston which destroyed the MAC and drove the plasma flow so as to generate the relatively large pressure signal seen at the GSAC.

Following the same hypothesis, since material motion at the tuff-grout interface may have been discontinuous, flow non-uniformities may have developed at the "rough" wall which might be seen as turbulence. Finally, radioactive materials may have moved along some of the block boundaries near the cavity and mixed with the grout early in its "turbulent" flow as indicated in Figure 4-2.

The preceding explanations suggest that LOS closure processes may be significantly degraded by block motions. But why should this scenario apply to MISTY RAIN when it apparently didn't to many other events and why has the DNA containment experience seemed to degrade with time as suggested by results shown on Figure 4-1?

4.3 A Possible Explanation for the Degradation of LOS Performance

DNA had a remarkably successful series of tests in the early part of the 1970's. Many people believed the containment problem had been solved. Then things began to deteriorate in spite of the fact that what were perceived by many people to be improvements were made in front end and in stemming designs. Other modifications described in Peterson, 1989, were also made during this time[1].

Another change also crept into the test configuration - one based simply on the geometry of the two-dimensional tunnel level. Conventional event siting criteria enforce a minimum separation between a proposed WP and those of previous explosions. In effect, this demands a certain minimum area be set aside for each test. When a tunnel complex is relatively new, these criteria coupled with

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[1] Of course, through the years many changes were made in test designs and emplacements. These included. for instance. pipe tapers, front end parameters, closure locations. drift cross-sectional areas. bedding plane orientations, etc. No one has yet found an explanation for our experiences based on these parameters. so the author .has sought an alternative.

engineering considerations involving tunnel layouts and constraints imposed by other planned events almost guarantees that working points will be relatively isolated. This is illustrated in the series of maps prepared by Dean Townsend, (1989) shown as Figures 4-3 through 4-12. These show the locations of many events conducted in n-tunnel relative to their predecessors. Note the increasing possibilities for prior shot influence as time goes by. Note particularly the increasing possibility for influence in the stemming region. The situation has changed qualitatively over the years.

As the hypothetical circular area enclosing a tunnel complex in the plan-view gets bigger, it is then geometrically possible to fit shots in between expended WP's. There is also strong economic motivation to do so in order to reuse access tunnels and diagnostic alcoves insofar as possible. Figures 4-10 through 4-12 show the configuration of the three events in n-tunnel leading up to MISTY RAIN.

The siting criteria faithfully recognize that shock-induced degradation of rock strength has routinely been observed in tuff out to ranges of the order of 2 R_c. This degradation has been
measured directly and illustrated by electron micrographs by TerraTek, (Torres, et al., 1989). It has also been demonstrated by Schmidt hammer tests and by insitu sound velocity measurements. As a result, the argument is routinely made that each new site is in essentially virgin ground since it is located well outside of 2 R_c from any previous event.

The point of the present argument is that homogeneous, shock-induced degradation is not the issue. Rather, we know that generalized block motion is a prominent feature of close-in ground motion. Therefore, the dominant factor which governs that motion is the nature of interfaces between various blocks of tuff. These interfaces may be faults, joints, fractures, or bedding planes. No systematic effort has been made by any testing organization to quantify these interface properties or to measure how they may be influenced by a nearby explosion.

Speculation leads to the unproven assertion that interfacial friction, if that concept is assumed to be the important characteristic, is degraded by shot-induced ground motion to a greater range than is material strength. Physically the asperities along an interface may be rubbed off by a little relative motion which may occur beyond the range of observable homogeneous strength degradation. It is conceivable that the ground-up asperities become clay-like and lubricate the interfaces. As a result, the next time the interface is stressed, it moves relatively easily. The TAPS failure and the n-tunnel extension collapse seen at MISTY RAIN may have illustrated this effect. It is also conceivable that motions induced in more than one direction by two or more nearby events are particularly effective in weakening sets of interfaces with different spatial orientations.

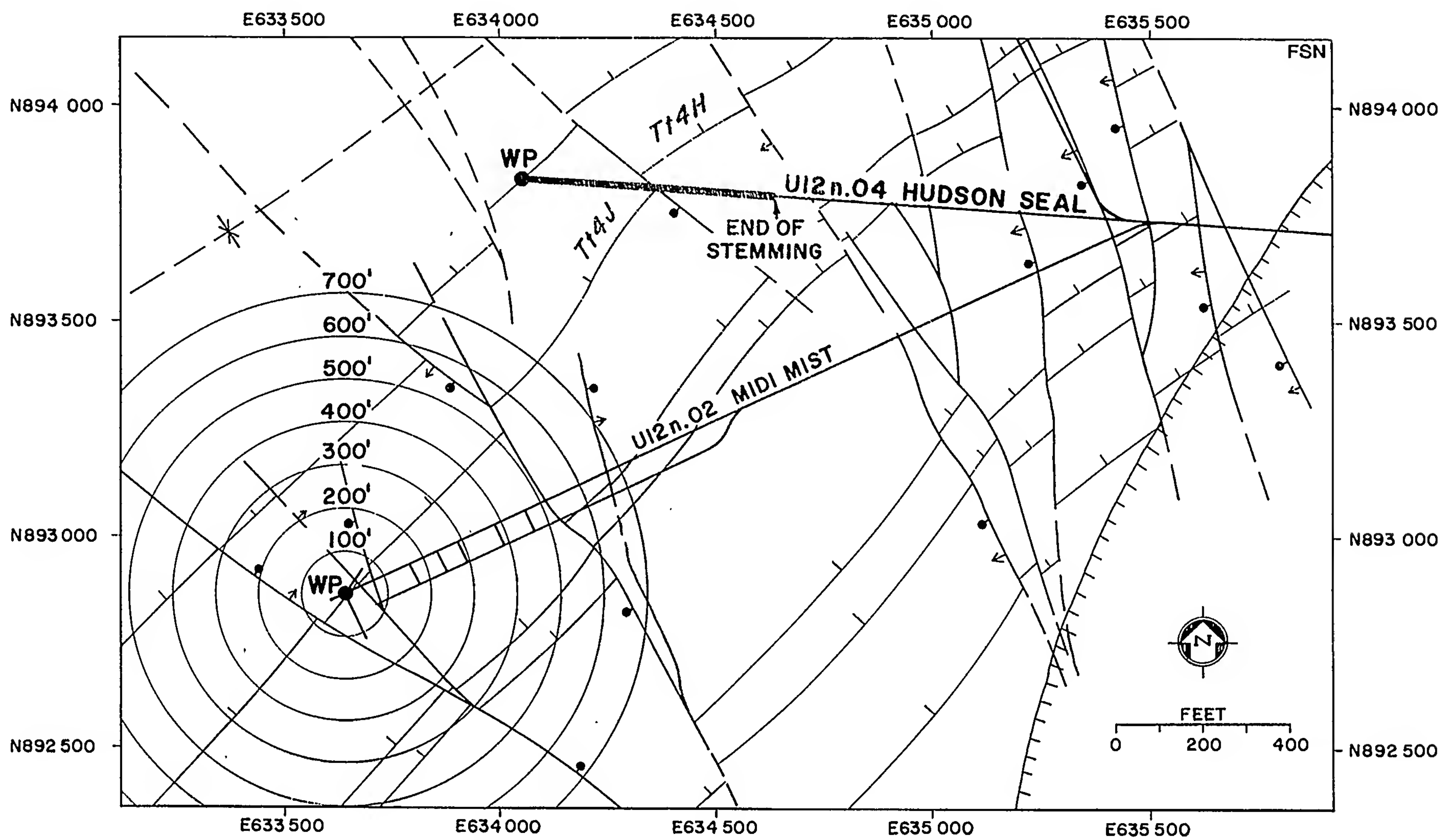

Figure 4-3. N-tunnel configuration at the time of HUDSON SEAL.

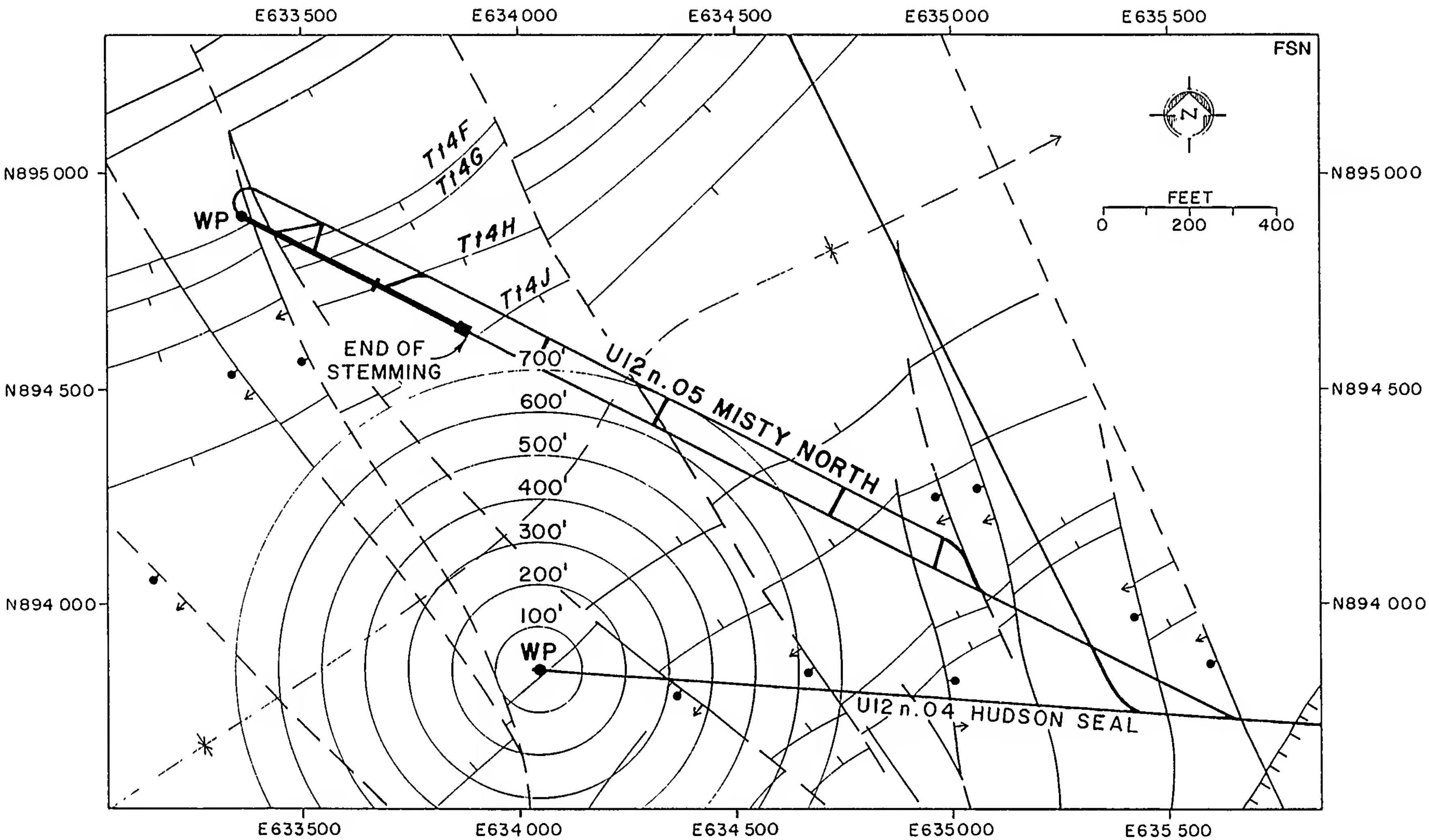

Figure 4-4. N-tunnel configuration at the time of MISTY NORTH.

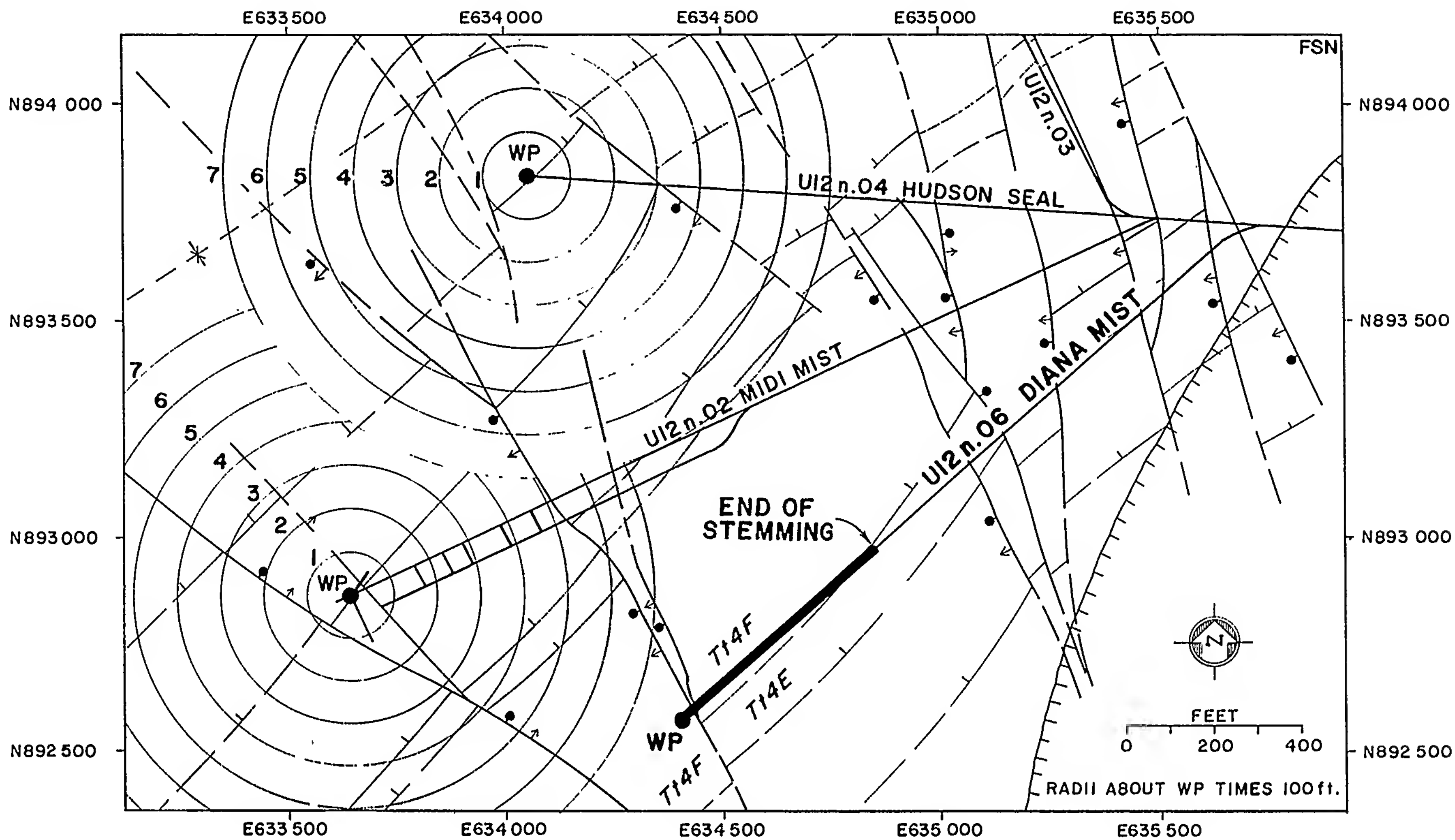

Figure 4-5. N-tunnel configuration at the time of DIANA MIST.

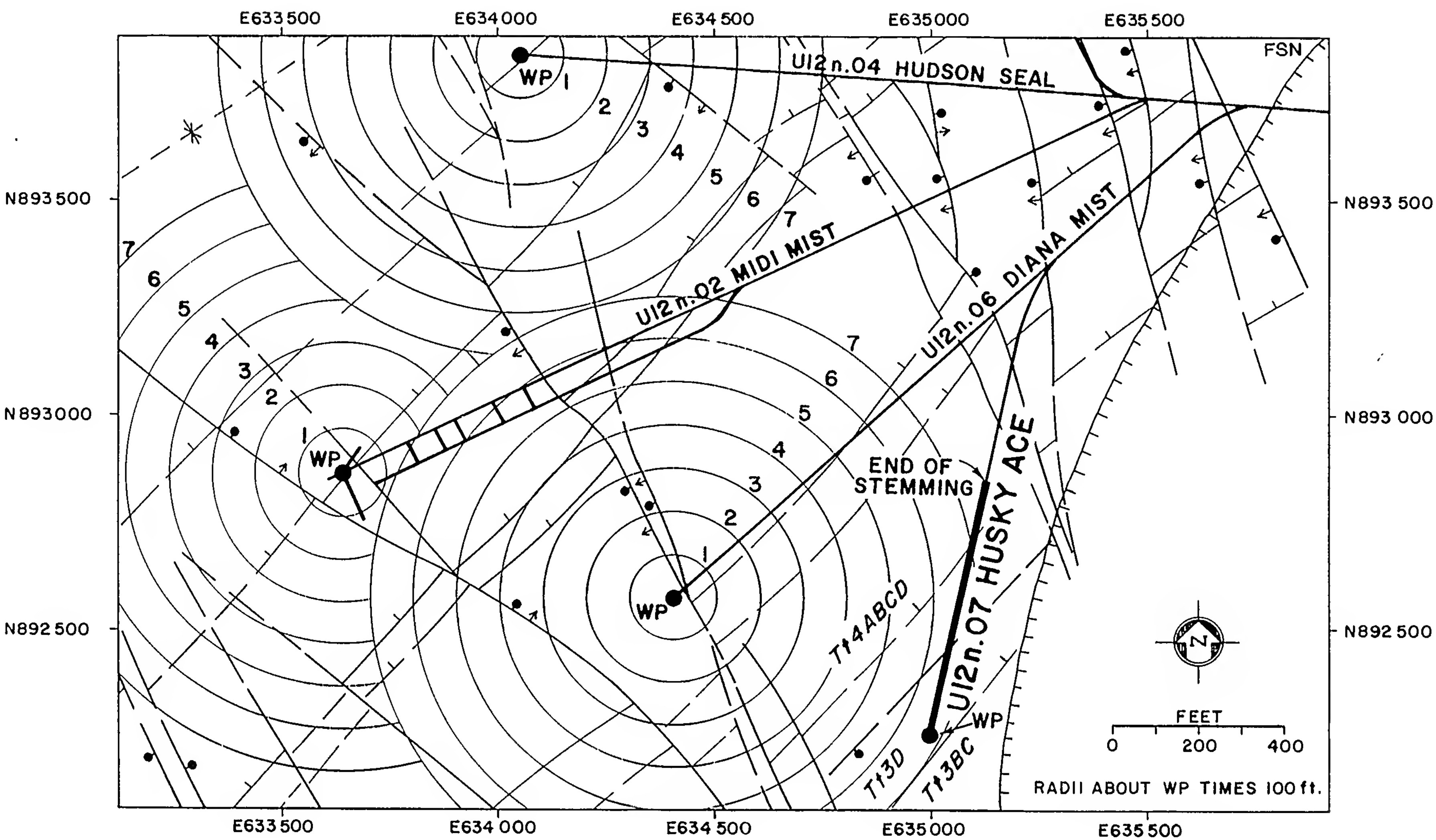

Figure 4-6. N-tunnel configuration at the time of HUSKY ACE.

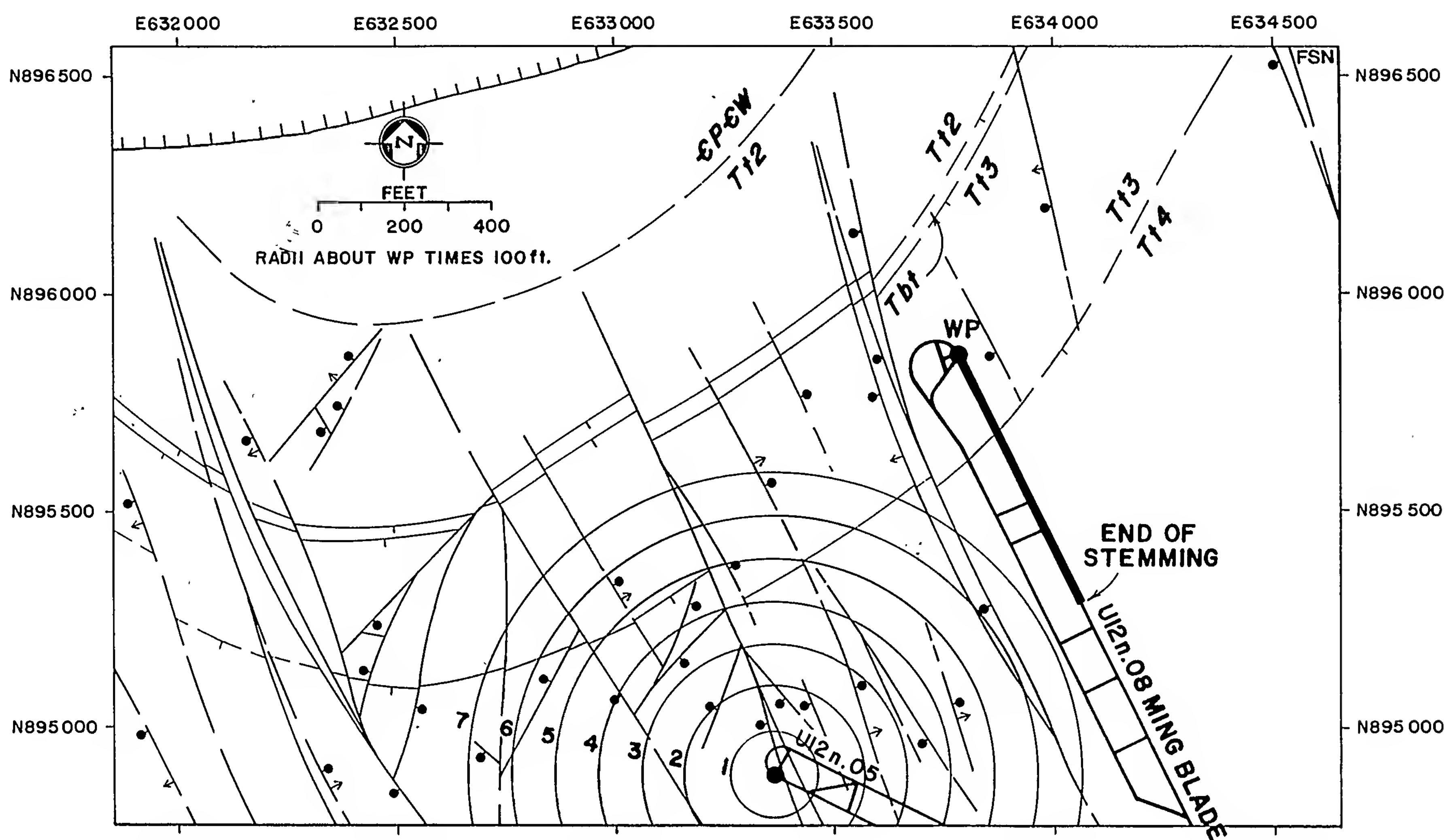

Figure 4-7. N-tunnel configuration at the time of MING BLADE.

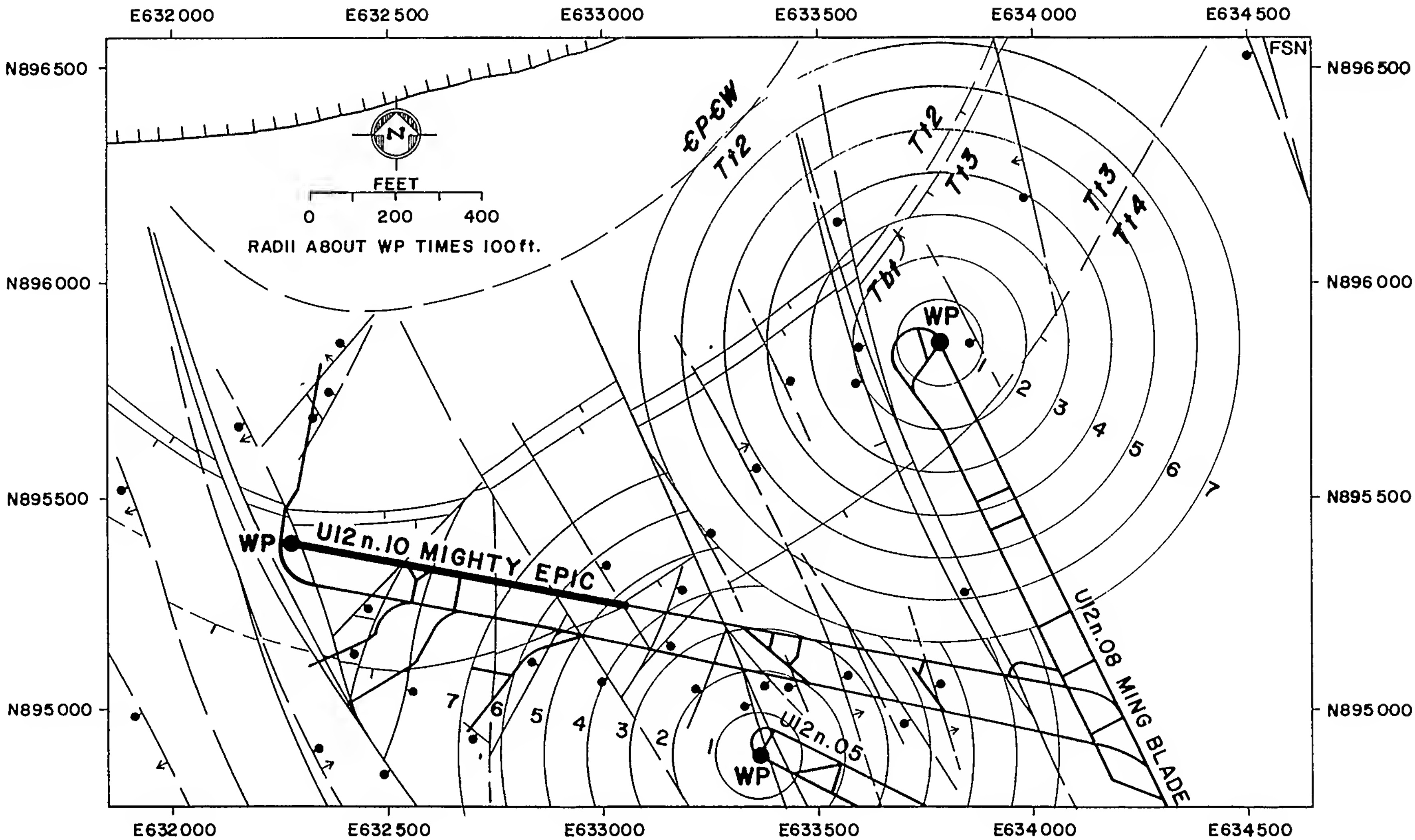

Figure 4-8. N-tunnel configuration at the time of MIGHTY EPIC.

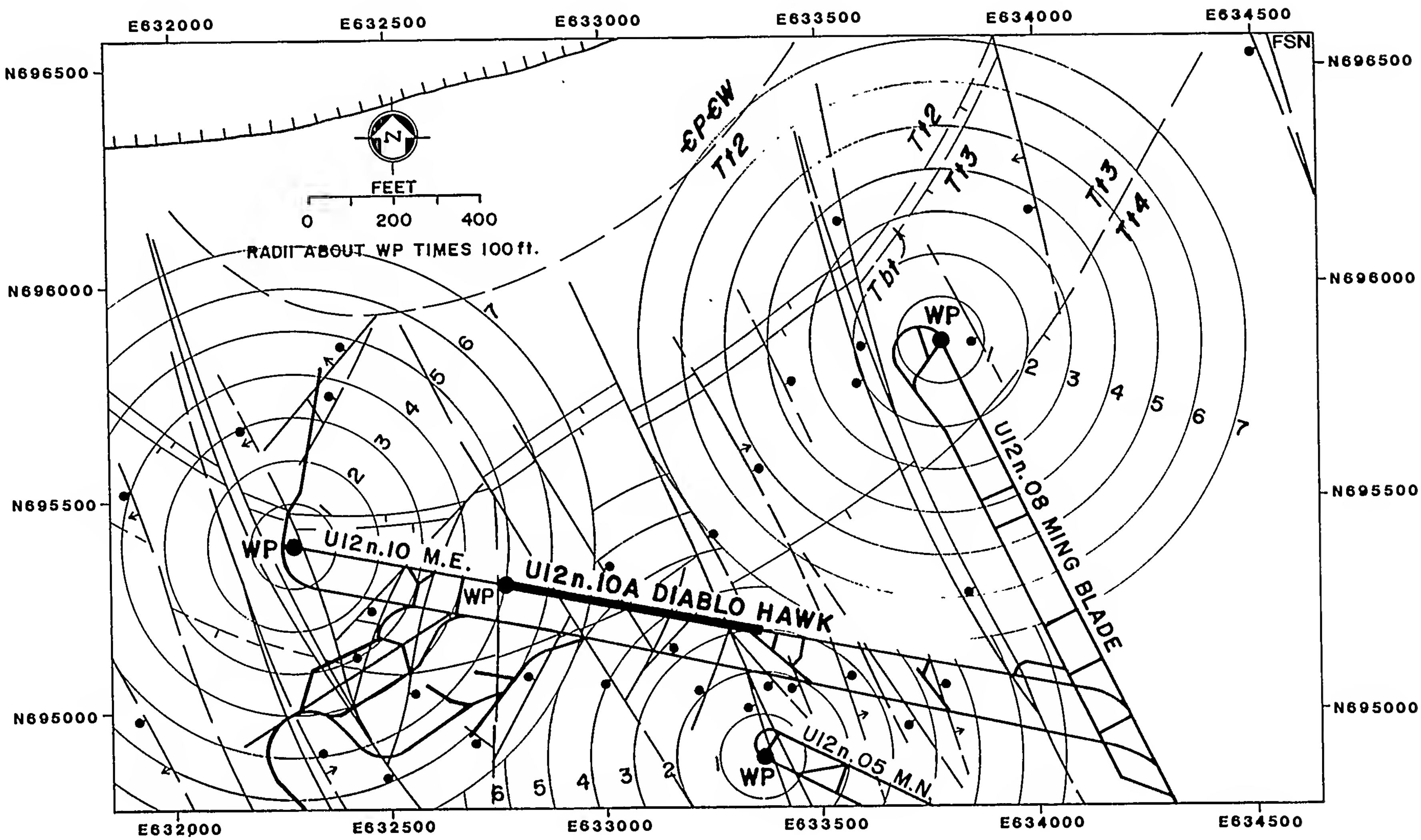

Figure 4-9. N-tunnel configuration at the time of DIABLO HAWK.

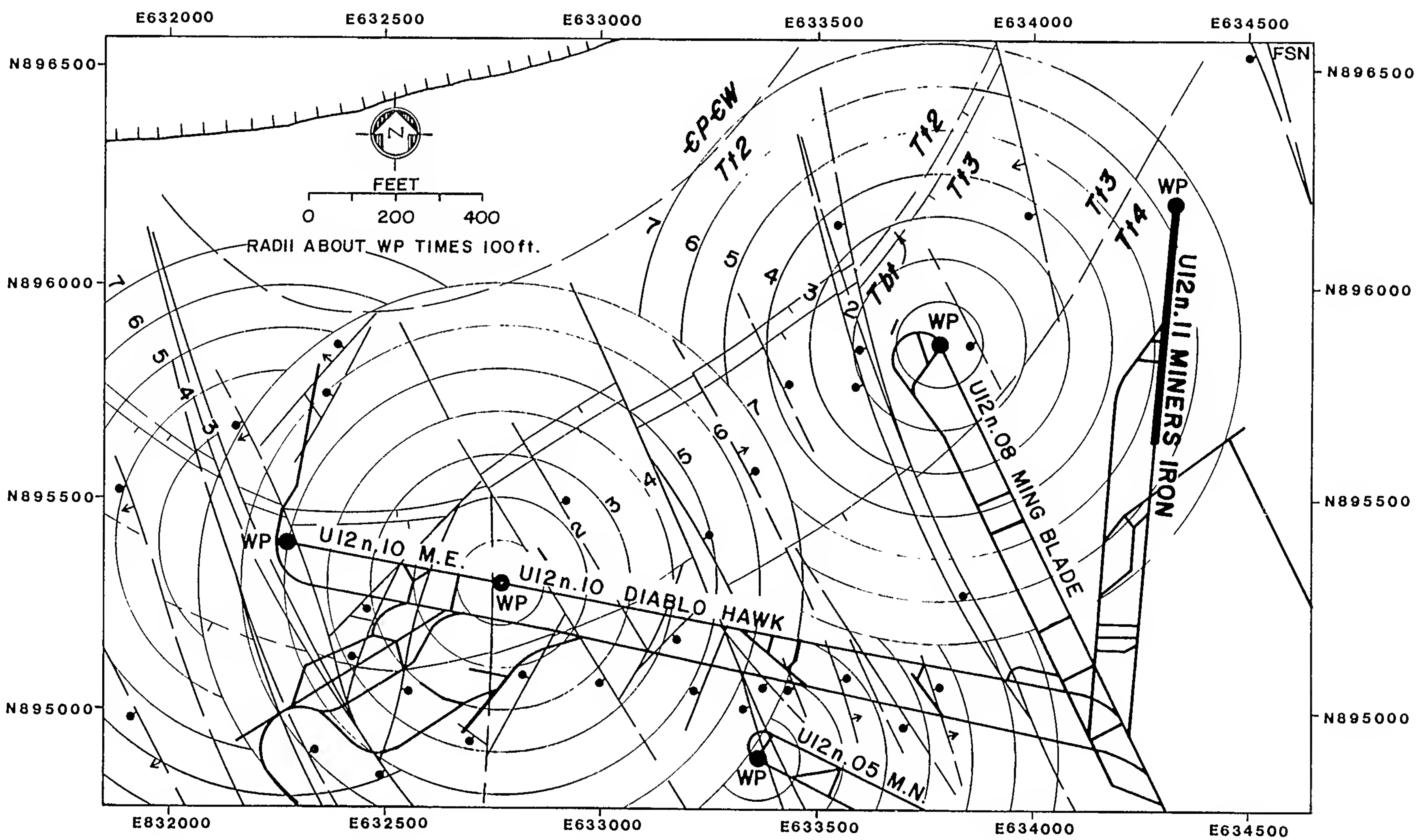

Figure 4-10. N-tunnel configuration at the time of MINERS IRON.

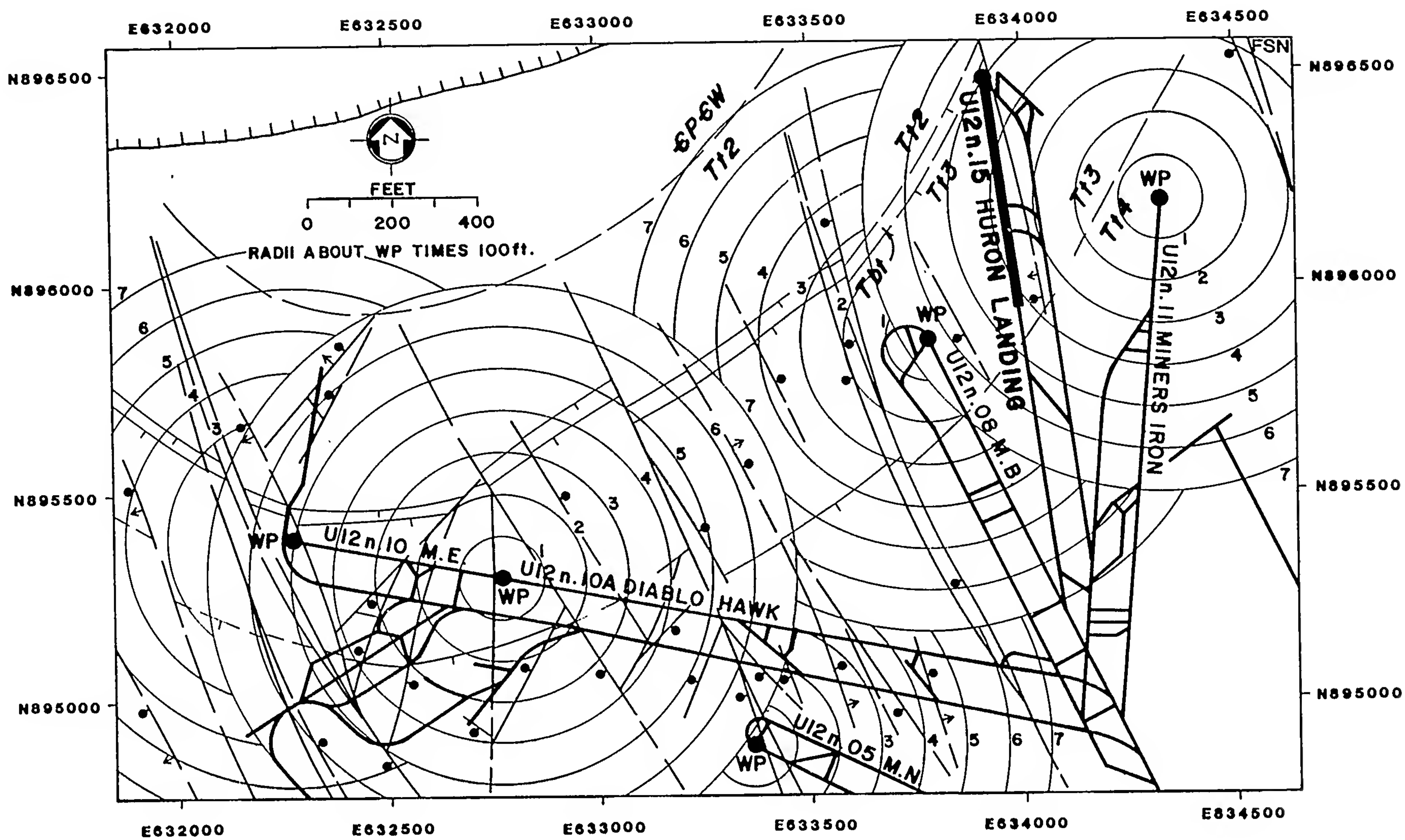

Figure 4-11. N-tunnel configuration at the time of HURON LANDING.

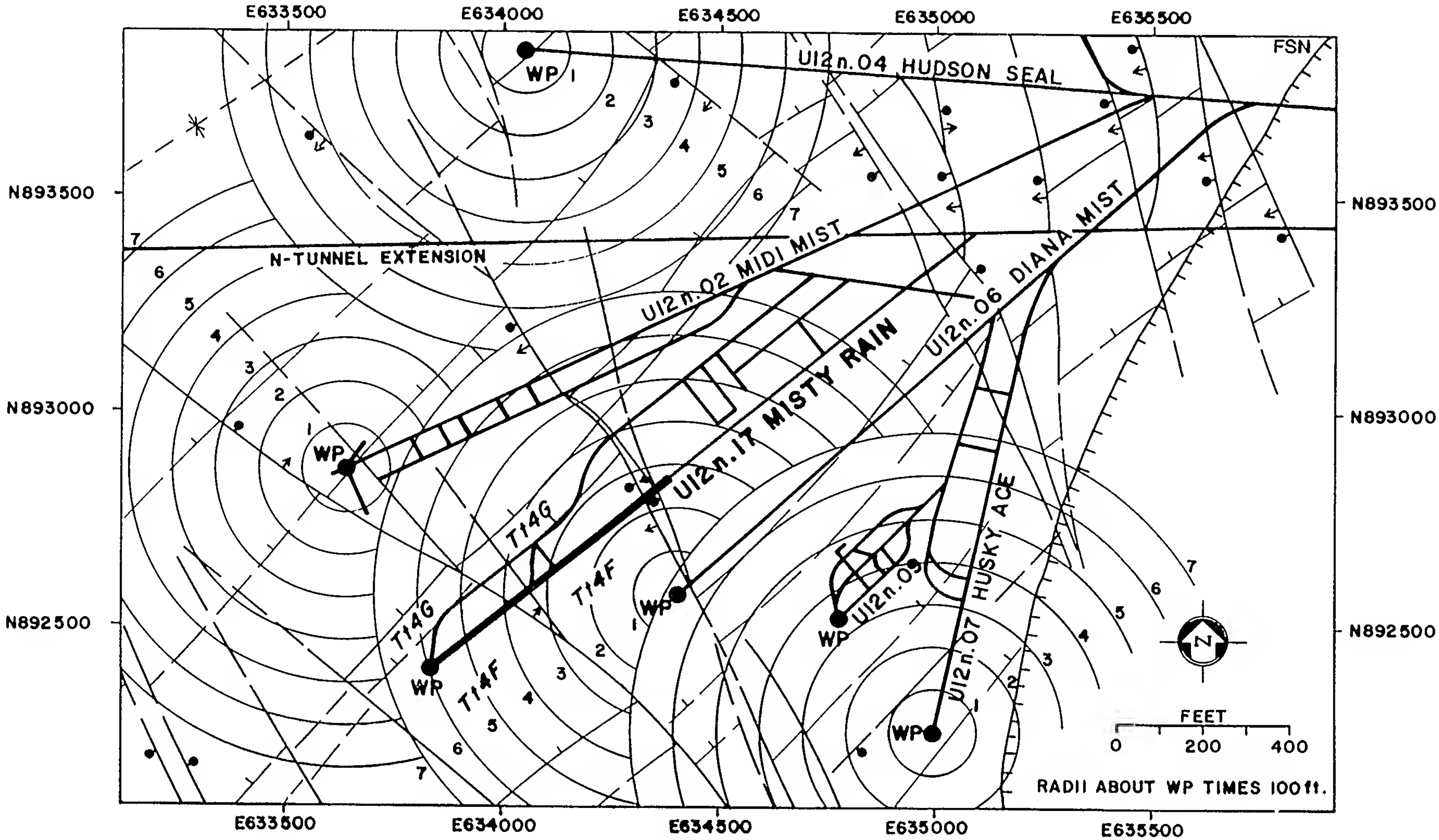

Figure 4-12. N-tunnel configuration at the time of MISTY RAIN.

Note that nothing has been said about the timing of fault motion. As far as prior shot influence is concerned, the fault motion of concern could occur at the time of shock passage or as a result of late-time stress relaxation.

As an example, the author believes that interfaces in the stemming region of MISTY RAIN were exercised and weakened by MIDI MIST and DIANA MIST, in particular. See Figure 4-12 for the relationship of the events. When MISTY RAIN was fired, a complex interaction involving the many interfaces present and their characteristics gave rise to a motion field similar to that sketched in Figure 4-2. Less close-in tunnel expansion occurred than expected, the grout motion was driven "turbulently" by discontinuous block motions, and radioactivity moving along one or more interfaces was mixed in. The assumed lack of tunnel expansion forced excessive grout flow and the failure of the MAC and GSAC. Late-time chemical reactions purged radioactive gases from the grout and provided the explosive mixture that caused tunnel and alcove damage.

MINORS IRON, I believe, provided different experience. In this case the MAC survived even though it was so close to the WP that some people expected it to fail. According to the present scenario one can speculate that in this case the close-in block motion allowed extra tunnel expansion and reduced grout extrusion. The different behavior postulated depended on so far unquantified differences in interfacial characteristics and the geometric relation between interfaces and the WP, and possibly the insitu stress field magnitude and direction.

There may be a hint of an *in situ* stress directional influence on this process to be found in Figures 4-3 through 4-12 if it is assumed that the direction of the regional in situ stresses relate to the prominent fault direction. Note that prior events in the vicinity of MISTY RAIN were located more or less along the prominent fault direction from the MISTY RAIN WP. There is also a weaker hint of this in the HURON LANDING siting. On the other hand, a line drawn from MINERS IRON to MING BLADE seems to be more or less perpendicular to the prominent regional fault direction.

MIGHTY OAK, whose performance as shown on Figure 4-1 was categorically similar to that of MISTY RAIN may or may not provide a contrasting example to the previously described phenomenon. Although maps similar to those shown on Figures 4-3 through 4-12 are not presently available for t-tunnel it can be seen on Figure 4-13 that MIGHTY OAK was emplaced at a site well removed from, but bracketed by prior events. In contrast to MISTY RAIN, the MIGHTY OAK separation from previous events is most similar to those shown on Figures 4-6 through 4-8 for the HUSKY ACE, MING BLADE, and MIGHTY EPIC events that performed well as indicated on Figure 4-1. The question is, what is the effective range of "interface strength" degradation in t-tunnel? If careful investigation shows that it is significantly larger than that in n-tunnel then the hypothesized explanation may also apply to MIGHTY OAK and if this is not the case then this explanation does not apply.

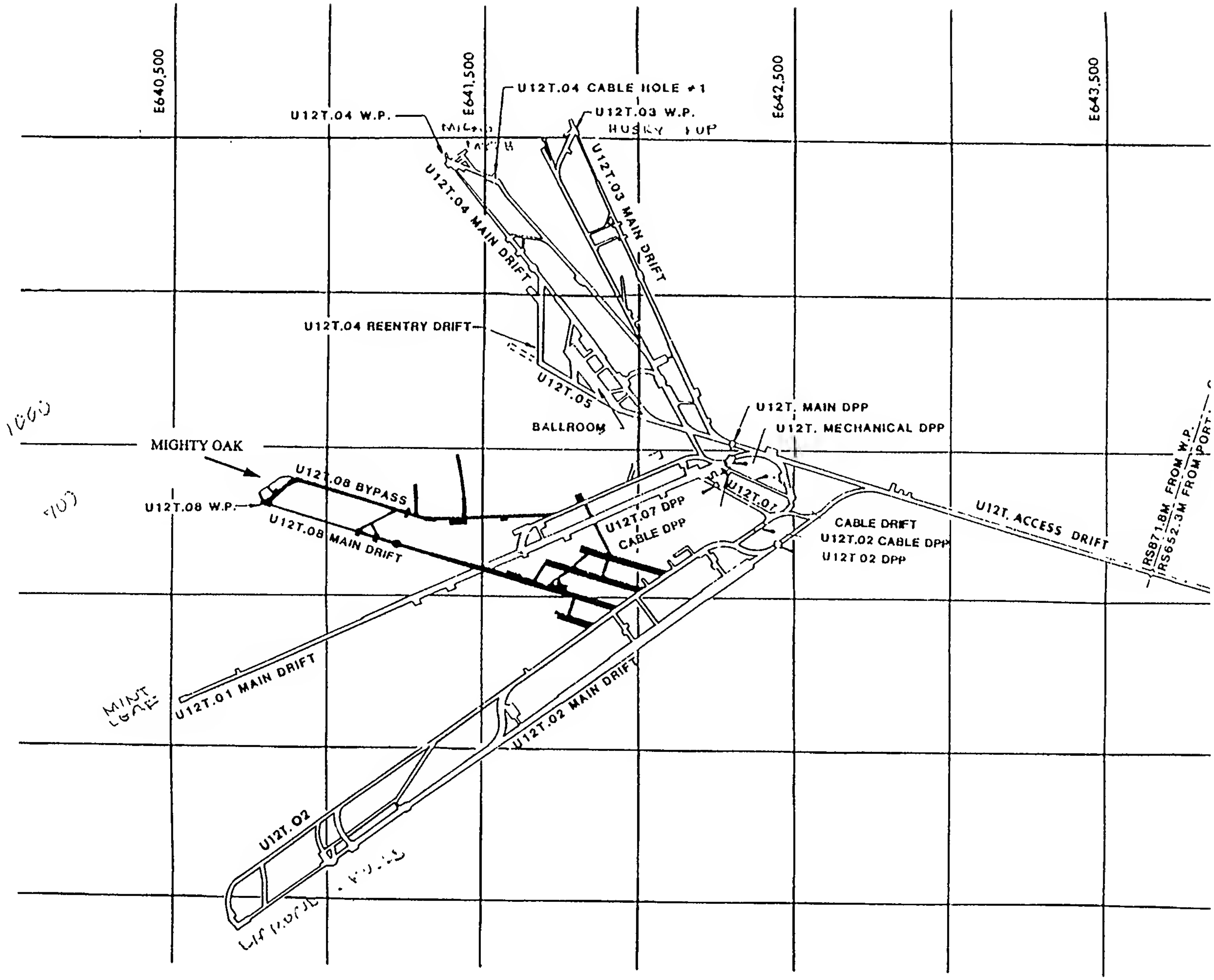

Figure 4-13. Layout of the t-tunnel complex showing the location of MIGHTY OAK.

4.4 Recommendations

The phenomenological scenario presented above postulates that interface characteristics are central to rock response, and since there doesn't seem to be any way to know the geometry and properties of all interfaces, the prospects for performing quantitative predictive calculations remain dim. A further serious complication is that the ground motion in even the simplest configuration is essentially three-dimensional. As a result there is little chance that conventional continuum models can be productively used to study the implications of the postulated scenario.

On the other hand, significant progress could possibly be made through the development and application of discrete element models which represent a material as an assemblage of deformable blocks separated by interfaces having prescribed strength and frictional characteristics (Cundall and Hart, 1989). Such a model could be used to study the implications of a number of configurations of potential interest. For instance, one could investigate the influence of *in situ* stress direction on stemming behavior for a number of interface situations. For a given *in situ* stress and stemming configuration one could find out which interface situations are particularly favorable or unfavorable to sample protection prospects. In other words, parametric calculations could be done to determine sensitivities to various emplacement and experimental variables. Such knowledge could help guide siting decisions. It could also help make general decisions concerning stemming plans and closure hardware. It should be repeated that such results would be generic. They would not be site-specific simply because one could not know all about all interfaces which might be important before a shot was designed. (It should also be remembered that this suggestion might not work out for some reason or other.)

Obviously, in order to make such calculations reasonably useful, one would need to know a great deal more about the geologic interfaces which exist in Rainier Mesa and how they may have changed as a function of nearby explosions. Getting such information won't be easy but there have been a number of laboratory studies of friction on rock joints (Bandis, 1990) as well as *in situ* measurements (Blejwas and Hansen, 1990) which use jacks to apply forces to large blocks of material.

Specific recommendations include:

1. Develop a three-dimensional discrete element code (Cundall and Hart, 1989) which can be used to study the stemming motion generated by an explosion occurring in a configuration defined by a particular array of interfaces and a particular *in situ* stress field. If an alternative to the discrete element method is found which is better able to treat this problem, of course, it should be used.

2. Develop methods to determine a technically adequate description of the mechanical properties of the several types of interfaces that occur between blocks

of tuff. These methods may involve laboratory techniques; they may involve field techniques.

3. Measure interface characteristics for a statistically significant distribution of interfaces that exist in a region which might be a shot point. It will be important to demonstrate the influence, if any, of multiple explosions on these characteristics.

4. Use the measured interface properties and the discrete element code in a parametric study of stemming behavior as a function of interface orientation and strength and insitu stress magnitude and direction.

5. Study the calculated results in an effort to determine favorable and unfavorable situations for sample protection and containment.

6. Until good new calculational data are available, future test designs should continue to be developed based on empirical data, previous experience, and proof test results with the results of conventional continuum mechanics calculations being used, as they have in the past, only as a qualitative guide.

5. SUMMARY

A hypothesis has been presented that the inhomogeneous ground motion driven by an underground nuclear explosion departs significantly from the uniform motion predicted by continuum mechanics codes. A residual stress may be formed, but that stress field should be expected to have a magnitude, location, and time of formation that is different than predicted by these models. As a result, it may not be prudent to use the residual stress concept as a pillar of "containment theory" since its predictions may not be reliable or conservative.

Quite possibly a "containment theory" based on the fracture theory of Griffiths and Nilson that ignores possible residual stresses would be more conservative, in better accord with observations, and less susceptible to the inherent uncertainties of the near field motion. As the development of such a "containment theory" proceeds, obviously it would be prudent to include the influence of block motions and residual stresses.

Furthermore, it is hypothesized that since the ground motion near an explosion cavity may be characterized by generalized block motion, the stemming response will be different then predicted by conventional continuum mechanics codes, particularly since the observed offset of faults is sometimes comparable to the meter-scale dimensions of importance in LOS closures. Perhaps the discrete element method can be used to provide good generic information useful for stemming and closure design. This approach, which views the material as an assemblage of discrete blocks, will however, require a better characterization of the strength of interfaces in the Rainier Mesa tuffs.

Finally, in view of the inherent complexity of containment processes and geologic emplacement media, we should continue to rely on empirical data, and relevant experience, and proof tests as the ultimate measure of proposed containment designs.

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Site Selection and Containment Evaluation for LLNL Nuclear Events

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ABSTRACT

During approximately the past decade, the site selection process at LLNL has evolved as the Test Program needs and resources have changed, containment practices have been modified, and the DOE and other regulatory agencies have become more restrictive. Throughout this period the Containment Program and the Field Operations Program at LLNL have managed a cooperative effort to improve site selection. The site selection process actually is three inter-related tasks, namely, selection of a stockpile hole for a specific nuclear test, selection of a drill site for a stockpile hole, and selection of a new drill site for a specific test. Each proposed site is carefully reviewed for known or projected geologic structure and medium properties, nearby holes, containment experience in the region, likelihood of drilling problems, programmatic need for a given depth of hole, and scheduling of Test Program events and resources. By using our data bank, our general knowledge of the Nevada Test Site (NTS) geology, and other information sources, as well as our background in drilling large diameter holes at the NTS, we have been able to optimize our use of NTS real estate and programmatic resources. The containment evaluation of a site is facilitated by considering the location before the hole is drilled. We will discuss imposed restraints and our criteria and guidelines for site selection and assignment of events to specific holes, along with the factors that influence selection of a Working Point (WP) depth. Since siting and containment evaluation are strongly related, most major factors related to the containment evaluation process will also be reviewed.

INTRODUCTION

There are three subsets of site selection as it applies to nuclear tests, namely: selection of an existing drill hole for a specific event, selection of a new drill site for a stockpile hole, and selection of a new drill site for a specific event because the stockpile does not contain a suitable site. These processes and the necessary information are overlapping, but differ in timing and in the available information at various stages. It is assumed that at any given site there is some combination of (non-zero) yield and DOB that will result in successful containment; whether the configuration is operationally feasible is another problem. The goal of siting is to optimize the various parameters so that operational feasibility and successful containment of yields of interest to device designers can be attained at a suitably low cost. This discussion is primarily directed to vertical emplacements, as opposed to tunnel events.

Many factors must be considered. The primary concerns are: (1) scheduling of field resources, such as drill crews and rigs and earth moving equipment; (2) event schedules; (3) shock sensitivity of a given experiment and possible interactions with other events; (4) depth range required for the working point (WP); (5) geologic structure (stratigraphy, faults, Paleozoic surface, etc.); (6) material properties, especially CO_2 content and particularly strong or weak materials; (7) depth to standing water; (8) potential drilling problems; (9) adjacent expended sites, craters, chimneys, subsurface collapses; (10) adjacent open emplacement holes or

unplugged post-shot or exploratory holes; (11) non-Test Program constraints, such as ground water concerns and roads and powerlines. Other constraints may be present; the above list is not all-inclusive.

Recently, since about 1990, there have been very few holes drilled compared with past activity, and the drilling capability available to Test Program is severely limited compared with past years. This circumstance has led to a rather different situation from past procedures (Olsen, 1983); now the existing stockpile must be analyzed for testing capacity, and the Nuclear Design community tailors tests to fit the holes, rather than the reverse, which had long been the predominant mode of operation. Containment evaluation, which combines site characterization with the dynamic processes accompanying a nuclear explosion and with the engineered features that go with the test, is an integral part of such an analysis.

We will review the process of site selection and the attributes of a "good" site from a containment standpoint and from an operational standpoint and will point out many of the numerous factors that must be considered in site selection. The process is mostly subjective; there is no checklist the completion of which guarantees a good site. While there is general agreement on "good" and "bad", there is often considerable diversity of opinion on the gray areas between. We also assume that no real site is perfect; any site can be made to have a release if one is careless or ignorant enough. There is also much subjectivity in deciding how many non-ideal factors, which by themselves are acceptable, when brought together make a site too risky and put constraints on utilization that are more stringent than normal. This paper will discuss the sorts of information available to the site selection process and the containment evaluation process, the difficulties and subjectivity involved, the trade-offs and constraints that must be considered, the guidelines that have evolved, and the process itself.

EXTERNAL RESTRICTIONS

Although not strictly a constraint on siting, the review of all proposed events by the Containment Evaluation Panel (CEP) is an ever present check on the siting process. The CEP is an advisory panel to the Manager of the U.S. Department of Energy Nevada Field Office, and is instructed in its Charter to "Ensure that all relevant data, technical information, and concerns available for proper evaluation are considered."

The CEP Charter is also the source of the definition of successful containment:

"Successful Containment: Containment such that a test results in no radioactivity detectable off-site as measured by normal monitoring equipment and no unanticipated release of radioactivity on-site. Detection of noble gases which appear on-site at long times after an event due to changing atmospheric conditions is not unanticipated. Anticipated releases will be designed to conform to specific guidance from DOE/DASMA (NV-176, Rev. 3, Planning Directive for Underground Nuclear Tests at the Nevada Test Site.)"

More direct requirements are the procedures handed down by DOE/NV that constrain siting or impose added restrictions or documentation. The strictest of these constraints, primarily in the form of USDOE/NV Standard Operating Procedures, are relatively recent in the chronology of testing, generally dating from about 1990. They are mostly driven by environmental concerns, particularly ground water protection. NTS-SOP-5413, *Characterization of Event Sites,* requires the Sponsoring Laboratory to ". . . conduct an investigation of the geology and hydrology necessary to characterize a site for containment of an underground

nuclear test." The SOP also requires that the U. S. Geological Survey (USGS) conduct an independent review of the sponsoring user's characterization and that the Director of the DOE/NV Test Operations Division be assured that the laboratory is in compliance.

NTS-SOP-5414, *Selection and Approval of Nuclear Test Locations,* sets forth environmental concerns, for example, endangered species protection, and the procedures to assure compliance with orders. The Sponsoring User must submit criteria letters for new drill holes or for construction at existing drill sites to the Director, Nevada Test Site Office - DOE/NV (NTSO). SOP-5414 Section 5.a requires the user to ". . . document their selection process for new emplacement holes or for nuclear tests in existing holes, and the criteria letters should summarize the selection reasoning . . ." No construction work at the NTS can be accomplished without the approval of NTSO.

NTS-SOP-5417, *Protection of Groundwaters at Nuclear Test Locations,* requires approval of the site by the Assistant Manager for Operations (AMO) of DOE/NV. SOP-5417 has several restrictions on siting, although it allows the AMO to approve a waiver. Restrictions (Section 4) are:

(1) Future testing should utilize previously used areas of testing.

(2) Minimize tests with working points at or below the water table. Testing within perched water conditions is excluded from this criterion.

(3) Working points should be placed no closer than two cavity radii from any regional carbonate aquifer.

(4) Emplacement holes should not be sited within 1,500 meters of the NTS boundary where ground water leaves the NTS.

(5) Emplacement holes which extend more than two cavity radii or 30 meters, whichever is greater, beneath the working point should be plugged to prevent the open borehole from becoming a preferential pathway for ground water contamination.

The SOP requires that the Director, Environmental Restoration and Waste Management Division (ERWM) reviews the user's siting documentation for compliance. If, during the drilling or construction phase at the location, the user discovers that the ground water level is " . . . different enough from that predicted to present a potential impact, the sponsoring user shall report this fact to the AMO within ten working days of becoming aware of the situation." The AMO will then determine if the hole is still acceptable for the originally intended use.

Other SOPs require that before use any area must be surveyed and determined acceptable with respect to archeological and historical sites, radiological control and other controlled or restricted regions.

The *Protocol to the Treaty between the United States of America and the Union of Soviet Socialist Republics on the Limitation of Underground Nuclear Weapon Tests*, commonly referred to as the TTBT or the Protocol, has potential impact on siting. According to the Protocol, conditions related to satellite hole location, void volumes, stemming density, and other conditions exist if the Verifying Party comes to the test. Conditions are different if a hydrodynamic yield measurement is made or if verification is limited to On-Site Inspection (OSI). Emplacement hole enlargements of more than 1 m (presumably on the radius) in the 15-m long zone containing the (12-m maximum length) device canister are prohibited if a

hydrodynamic yield measurement is to be done. There are also limitations related to siting of multiples. Some topics are vague in the actual document, and concerns will need to be resolved by mutual agreement on a case-by-case basis as they arise. In any case, the Treaty can affect siting, and restrictions in the Protocol must be considered if verification by the Russians is possible. The treaty specifically honors the testing party's containment practices, so no deleterious restrictions from a containment standpoint are likely to surface.

OPERATIONAL ASPECTS

From an operational standpoint there are a number of factors that influence site selection independent of containment concerns in addition to the DOE restrictions. For any given site it is virtually impossible to have everything as one would wish, and trade-offs and optimization are required. Operational concerns are: avoidance of damage to power lines or major roads; minimization of earth moving necessary to make the site ready for use; proximity to power and water; separation from existing holes that must survive ground-shock; amount of cementing required to plug adjacent open holes, usually post-shot holes; history of hole caving in a region; proximity to the standing-water-level/need to install a liner or intermediate casing in a hole; size of canister to be emplaced in the hole; need for hole straightness / history of hole deviation in an area; shock interaction from other events on the drill rig, on event hardware being assembled or emplaced, and on the open hole itself; and preference for Yucca Flat over Pahute Mesa for financial reasons and for weather.

The operational concerns are just as real as the containment concerns, although they are, in general, somewhat more clearly defined. Operational problems tend to be related to man-made items and are, therefore, more manageable than, say, the geology in Area 2. Operational constraints and NTS-SOP that address site selection will not be discussed in the same detail as containment interests, but will be covered in sufficient detail to give a complete view of site selection. Additionally, operational concerns tend to be more temporal than containment issues: a 20% CO_2 content will not go away; a warehouse or road might be relocated. The scheduling aspects of siting are primarily the responsibility of Field Operations and will not be mentioned in any detail here.

WHAT IS THE THREAT?

There are four general types of containment failure against which we must guard. They are:

(1) LOS-Pipe Releases. This category is not currently relevant for vertical tests, but was one of the original forces in establishing containment as a necessary part of the Test Program. These releases were either dynamic vents or seeps, but in each case the path (or a significant portion) was through a LOS pipe to the surface for vertical tests, or to unstemmed tunnel for horizontal tests. These LOS pipes were for effects testing, not ordinary diagnostics, and are now almost exclusively the province of the DNA tests in Area 12 tunnels.

(2) Stemming and Gas-block Failures. These releases were most often seeps and were the most common source of failure. The release could be due to failure of an engineered feature or to the simple absence of something, such as the lack of cable gas blocks during the 1960s. We have made major strides in preventing these releases, although one of the rare post-BANEBERRY releases, and the only one detected off-site, RIOLA, was due to

a stemming failure. The key to the first two classes is that they are essentially man made, and we have, in principle, more control.

(3) Seeps not through a Man-made Channel. These are releases that may or may not be localized; the source may be at one or a few "hot spots" or it may be diffuse, such as "the crater". The blame is most often set at noncondensable gas, generally CO_2 from the medium.

(4) Dynamic Vents through the Medium. These are rare but dramatic; the population is BANDICOOT, WICHITA, PIKE, PINSTRIPE, and BANEBERRY for vertical events, (Schoengold, et al., DOE/NV-317) and PINSTRIPE was found to have part of the path through a LOS pipe. In a technical sense we might include the cratering events in this class, since they provided valuable information on scaling, but their behavior was expected and not considered a containment failure. These ventings were the result of putting something in the wrong place and are critical examples of a siting error. Note that the site, *per se,* may have been acceptable if a different "something" had been sited there.

There is often no clear-cut division between these classes except for dynamic vents, which are obvious, and seeps, which must be detected instrumentally. PINSTRIPE, for example, was a combination of #1 and #4, and CO_2 (#3) has often been associated with #2. The main point is that an acceptable site is necessary, but man-made features are what stress the site. A given location may be fine for one test but not so for another. It is assumed true that there is some non-zero yield that can be successfully contained at any practical site and that there is some yield that can stress a site to failure no matter how good the site may seem. Interestingly, some low yield may be a more severe test of a particular site than a higher yield, although an unlimited yield increase will always result in eventual failure.

CONTAINMENT ASPECTS OF SITING

In its simplest form, the containment concerns of a site revolve around the geology (structure and material properties) and the man-made features close enough to be of concern and how the given site responds to the explosive source. The containment concerns are, in large part, subjective: there is no firm criterion comparable to 1500 m from the NTS boundary. It seems intuitively clear that a large open hole near a proposed WP is unacceptable without remediation. It is not at all clear what "near" means or how far must one go so as to no longer be "near." For this reason there are rules-of-thumb that have been developed over the years. These guidelines are generally just that, "guidelines", rather than firm prescriptions, and they have varying degrees of experience and analysis as their bases. Since there are few, if any, agreed upon quantitative containment criteria, the reader will notice that such terms as "major", "minor", "significant", "typical", "potential", and "near" are often not elaborated on, since there is sometimes little agreement when one tries to quantify such terms and since meanings can alter with time as our experience and understanding evolve. We will make an effort to be as specific as is possible, but some vagueness is unavoidable due to lack of firm definitions and to definitions that change with time, and because a specific circumstance could be perfectly acceptable in one instance and of concern in a different context.

The question of adequate site characterization and the level of detail necessary is difficult and dependent on the situation at hand. The site characterization mandated by NTS-SOP 5413 is certainly a minimum for an adequate containment analysis, since that has no consideration of the dynamic response of the site to a test. One would like to have a "complete" description of the site

to at least one depth-of-burial (DOB) away from the WP. With the exception of seismic, magnetic, and gravity techniques, we do not have operational diagnostic tools capable of "seeing" more than a few metres beyond the hole wall, and all of our techniques do some form of averaging over the volume they purport to measure. Since we would not really know what to do with a *complete* description, say one that supplies values for all properties of interest to anyone on whatever scale is requested for all material within the 1 DOB radius cylinder around the emplacement hole to at least 2 DOB depth, we must decide what is actually feasible and adequate. Adequacy is in the eye of the beholder; a geologist and a computational physicist who is setting up a model to calculate event behavior would certainly focus on different aspects of the site. The Containment Scientist will, of course, have a different view, as would an engineer designing emplacement hardware or a stemming plan. The non-site-dependent factors, especially the explosive yield in question, obviously have a major impact and, to some extent, provide a rudimentary scale by which to view the situation. The cavity radius and the pulse width of ground shock/motion give some idea of the scale to be considered. These parameters are not fixed; for example, the shock pulse width generally increases with time, and it seems reasonable that a coarser description of the environment further from the WP may suffice.

A. Geologic Structure

Geologic structure of concern to containment is the spatial distribution of geologic materials of various properties. The emphasis from a containment standpoint has shifted somewhat over the years as our understanding of event phenomena has evolved. In the early stages of underground testing, in the 1960s and, to some extent, even in the late 1950s when the first underground tests were conducted, the primary focus related to geologic structure was on faults. (Remember that there was no statutory requirement to contain tests until the Limited Test Ban Treaty took effect in the fall of 1963.) Faults are, by definition, planar zones of weakness. It was intuitively obvious that such features should be avoided, since containment depended on the performance of some type of pressure vessel and a built-in weakness was undesirable. Of particular concern were the "major faults" typified by the Yucca Fault. As time went on and as our capability for site characterization improved, it became clear that "minor faults" were common features in the Tertiary volcanic rocks that comprise a major portion of the testing environment. Literally hundreds of faults, mostly small, have been mapped in the tunnels in Rainier and Aqueduct Mesas. Downhole photography, not routinely available in the early stages of underground testing, revealed numerous faults in some emplacement holes, and occasional faulting was observed in alluvium. There are no cases in our experience where a release was considered to be due primarily to the simple presence of a fault; there are, however, several cases where a fault was contributory, such as PINSTRIPE and BANEBERRY, and possibly BANDICOOT.

Experience and calculational modeling have indicated that the primary concern with a fault is not the plane of weakness but the acoustic/mechanical mismatch generated when materials of different properties are brought together by the offset. Faults that have small contrasts in material properties across the fault plane apparently have trivial impact on event phenomena. A fault loaded by a shock front impinging on the fault plane is, in general, held closed rather than ripped open along the plane of weakness. There may be some differential motion on the fault plane driven by event induced ground motion, but it is seldom of containment significance. Such shot-induced displacement has been clearly observed in the Area 12 tunnels. BANEBERRY is one instance where such behavior has been interpreted as playing a role in the

venting. The importance of faults in the near region around a proposed shot location has evolved to a concern over material-property mismatches whether they are the result of faulting or of stratigraphic contacts. The plane of weakness, *per se,* has not been found to be a significant threat. Our experience has, however, been restricted to "minor faults"; we have avoided major structural features such as the Yucca Fault. Where the fault is a relatively large zone of weak material (a mismatch in itself) the fault could be a significant hazard, but we have no direct evidence of the response of a major fault to a closely adjacent explosion.

Faults with significant surface expressions are generally avoided for operational reasons as well as for containment concerns. The main faults in this category are the Yucca Fault, the Carpetbag Fault, and the several named Basin and Range faults on Pahute Mesa. Fault scarps are generally avoided because of the extra earth moving and inconvenience in dealing with a discontinuity on the surface and because of the likelihood of additional offset on the fault accompanying an event. All such faults at the NTS have a generally north-south trend.

In practice we now essentially ignore small faults. A small fault is one with little or no fault gouge, is thin and tight, has an offset that does not exceed a few metres, does not present a significant acoustic mismatch, and does not display significant offset in alluvium. We have tended to stay a depth of burial away from major faults for depths up to a few hundred metres, but there has been successful experience in Yucca Flat with events as close as 355 m, BILLET in U7an at a DOB of 635 m, and two events with yields in excess of 50 kt that were 8.4 measured cavity radii to the west of the Yucca Fault and one event in the same yield range that was 7.6 measured cavity radii. These events were all successfully contained. Although there is no direct evidence, there is a general, though not necessarily strong, feeling that there is more risk associated with siting on the up thrown side of a major normal fault. The argument is that there is a tensile state tending to open fractures parallel to the fault on the up thrown side, while the down thrown block may be under compression, in a relative sense. The events just cited were on the up thrown side, so presumably siting at least that close on the down thrown side is safe. Normal faults dip toward the down-thrown side, so one must take into account the difference in proximity at depth as compared with the surface. BANEBERRY and PINSTRIPE had faults identified as being near one cavity radius from the WP; these events both resulted in dynamic venting, but each also had other negative features in addition to the mere presence of the fault. In PINSTRIPE there was a large line-of-sight (LOS) pipe that provided part of the release path, and BANEBERRY had a large region around the WP of altered tuff with a high montmorillonite content that was virtually saturated and mechanically very weak. BANEBERRY was a case where the faulting had brought a large mass of high-density, high-strength Paleozoic rocks near the WP that had a strong deleterious effect on the stress field around the WP.

A desirable feature of a site is uniformity. A fairly wide range of material properties has been found to be acceptable if major discontinuities are absent. Very high strength rocks are less desirable, since they can lead to extensive fracturing that is not closed up again and since such rocks tend to have little gas-filled porosity, which serves as a reservoir for radioactive gas. Uniformity is desirable because the radial motion away from the growing cavity is not perturbed. The motion tends to overshoot its equilibrium position due to the elastic properties of the material, and the motion reverses, "rebounds", and returns to a stable configuration. If all motion has been radially outward, the rebound results in convergence and formation of a high hoop-stress around the cavity. The residual hoop-stress is (hopefully) higher than the cavity pressure, and failure of the cavity pressure-vessel is prevented. If heterogeneities in the material surrounding the WP distort the radial motion, the rebound will be asymmetric and may not be

convergent, and the residual stress will be degraded or even absent. A homogeneous environment is most desirable; the details of the material properties are secondary, although the necessary DOB is somewhat dependent on the material. An obvious question, of course, is: "How much heterogeneity is acceptable?" That question is addressed computationally as well as experientially, and again brings up the need to determine the detail necessary in the computational models (zoning) and material descriptions. Here one must optimize the combination of validity of the model results, the ability to run the problem(s) in a reasonable time, and the ability to describe the environment being modeled.

B. Material Properties

There are numerous properties of the earth materials that can be determined in one way or another, but we have yet to discover a property or simple combination of properties that could be considered a "containment" parameter. Our knowledge of physical properties comes from analyses of material samples, from geophysical logging methods, and from deductions based on measured event phenomena.

Samples have been obtained in at least five ways from drill holes, namely: drill cuttings, core, Hunt sidewall, sidewall coring, and percussion gun. Tunnels have used analogous techniques, but the process is clearly simplified by being there. Samples are (or have been) used for lithologic studies, CO_2 content, H_2O content, grain density determination, x-ray diffraction, strength measurements, bulk density measurements, porosity measurements, NMR measurements (discrimination of bound water from free water), permeability measurements, radioactive species contents, and more exotic one-of-kind studies. Most of the determinations just mentioned require *in situ* core, which has been carefully handled to preserve structural integrity and water content, etc. Such samples are expensive to obtain, and are often very difficult to obtain, especially in either very hard or friable formations. The simplest samples to obtain are cuttings samples, which are routinely gathered at 10 ft intervals during hole drilling by simply collecting a sample of the drilling fluid returns and removing the water. These samples are certainly not representative of the native material, but they have been found to be useful, in general, for lithologic descriptions, CO_2 determinations, grain density determinations, and x-ray diffraction analyses, where "averaging" over the cutting face of the drill bit seems acceptable. Cuttings samples are also susceptible to contamination by hole sloughing and are sometimes suspect due to poor quality control.

Sidewall samples are more expensive to obtain than cuttings, since a dedicated trip to the hole is required. Hunt sidewall samples were, at one time used for a number of routine material properties determinations, but they had several flaws. They were subject to invasion by drilling mud and drilling fluid, they preferentially sampled softer formations, and they were relatively expensive. Percussion gun samples are the easiest to obtain aside from cuttings, but they are small, and sampling success is dependent on the nature of the material. They are useful for qualitative studies and, to some extent for quantitative studies, since they can be (in principle) obtained at a given horizon, and replicates are relatively inexpensive; they, too, are difficult to obtain in very hard or in unconsolidated material.

Geophysical techniques have been used to determine bulk density (gravimetric and gamma-gamma), water content (the epithermal neutron log), electrical conductivity (E-log), natural gamma-radiation level and spectral-gamma measurements, elastic wave propagation speeds (compressional and shear), seismic reflection/refraction surveys, C/O measurement,

remanent magnetism, and a few exotic measurements. Several of these have been used more or less routinely for material property data acquisition, particularly the gamma-gamma density tool and, more recently, the epithermal neutron tool. Other tools are used as an aid in stratigraphic determination, such as the E-log, the magnetic log, and natural gamma log. The only methods that give information beyond a range of a few metres are gravimetric, magnetic, and seismic surveys; there are electrical surveys that in principle have extended ranges of investigation, but they have not been used with any regularity at the NTS.

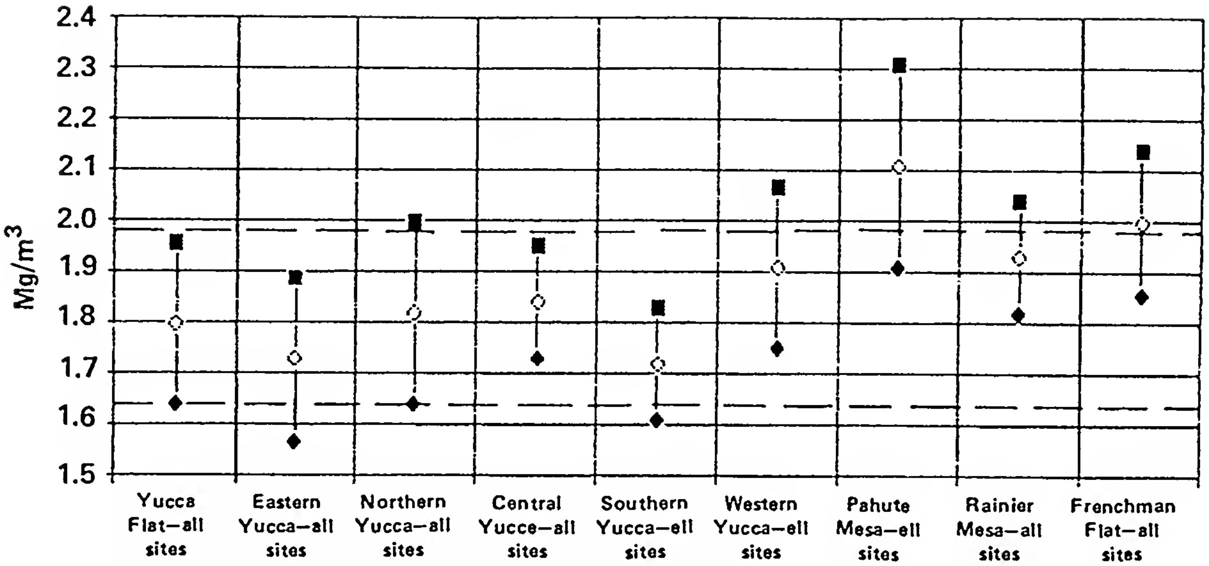
A basic question is: "How representative is a sample or log value of the property to be determined, and how well does the property need to be known?" Some properties need to be determined with reasonably good accuracy, at least for some applications. The most common example is that of having to calculate a parameter using the difference of two nearly-equal numbers where no direct measurement technique is feasible. This is a routine problem when calculating saturation and gas-filled porosity using measured bulk- and grain-densities and water contents. The absolute value of the grain density, for example, may be an important quantity to the geologist attempting to understand the mineralogy of the site. On the other hand, event phenomena are rather strongly dependent on gas-filled porosity, but insensitive to the grain density, *per se,* used to calculate it. The absolute bulk density near the WP is important because it is a measure of the mass around the WP and, hence, influences the extent of vaporization and melting. Farther from the WP, the bulk density contrasts at an interface are likely to be more important than the absolute values, since they affect shock propagation.

We have developed a framework of what is considered normal material for various locations and geologic units at the NTS. If a material/site is found to be comfortably in the population of good containment experience, we accept that as evidence that the site is acceptable without a detailed, quantitative analysis of the expected event behavior. Such a treatment is not necessarily desirable from a scientific standpoint, but it has been generally successful and has functioned where the resources have not been made available to do a more rigorous analysis. Such a mode of operation has been found to be considerably less expensive that the more technically sound approach, but it forces containment analysis to be more conservative than might be the case with better analytical tools, and it makes analysis of an atypical site much more difficult. The site that does not fit good experience is, of necessity, somewhat suspect, when the problem may well be with our tools (or lack of same) rather than with the site.

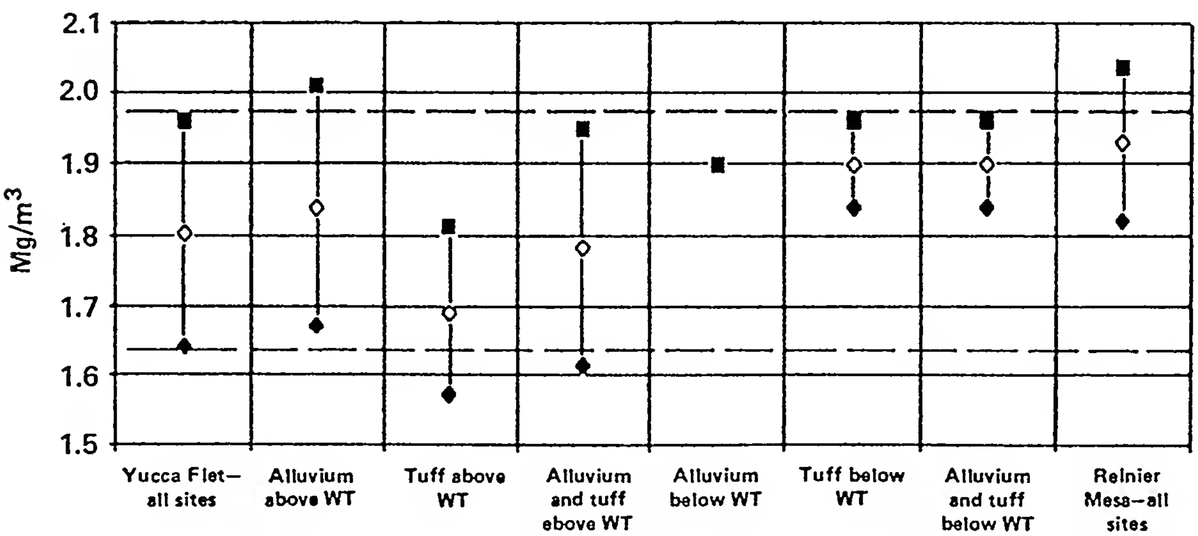
The geologic setting is considered for every proposed event; the Containment Prospectus and the CEP presentation describe the geologic setting of the test under consideration. What constitutes typical geologic material has been most thoroughly studied generically by Howard (1985) and, more recently, by Burkhard (1989) at LLNL. The same topic has been studied for restricted areas at LANL with respect to the Sand-Pile Concept (Fernald, Brethauer, & Dixon, 1974) and the Tuff-Pile Concept (1983). The LANL used the generalized properties to project values at sites within the region of study, thereby avoiding the expense of logging and sampling at all individual sites. Figures 1-3 (from Howard, 1985) show the ranges of some physical properties for various classes of materials; Table 1 (from Burkhard; 1989) shows mean properties and standard deviations for stratigraphic units commonly encountered at the NTS.

The problem is that the materials may be perfectly normal in a geologic sense, but the particular configuration, including credible explosive yields, may lead to untoward behavior. Merely describing the site and determining that it is not geologically anomalous is not sufficient to ensure satisfactory containment. Additionally, a given site and yield combination can result in

Figure 1



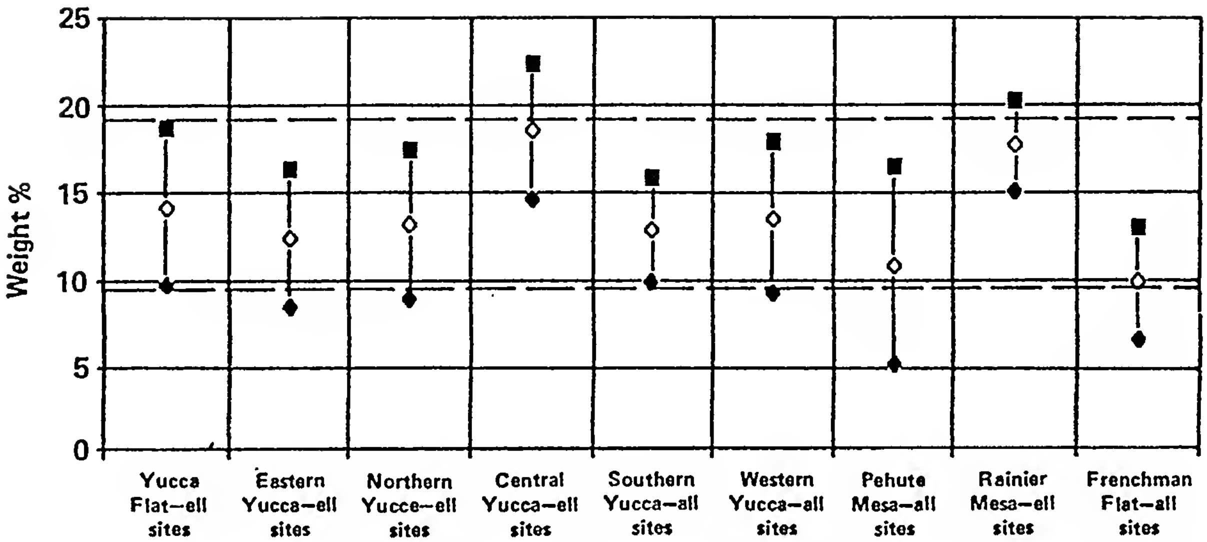
Working point density—all areas. Dashed lines represent the range of ±1 standard deviation for all Yucca Flat sites.



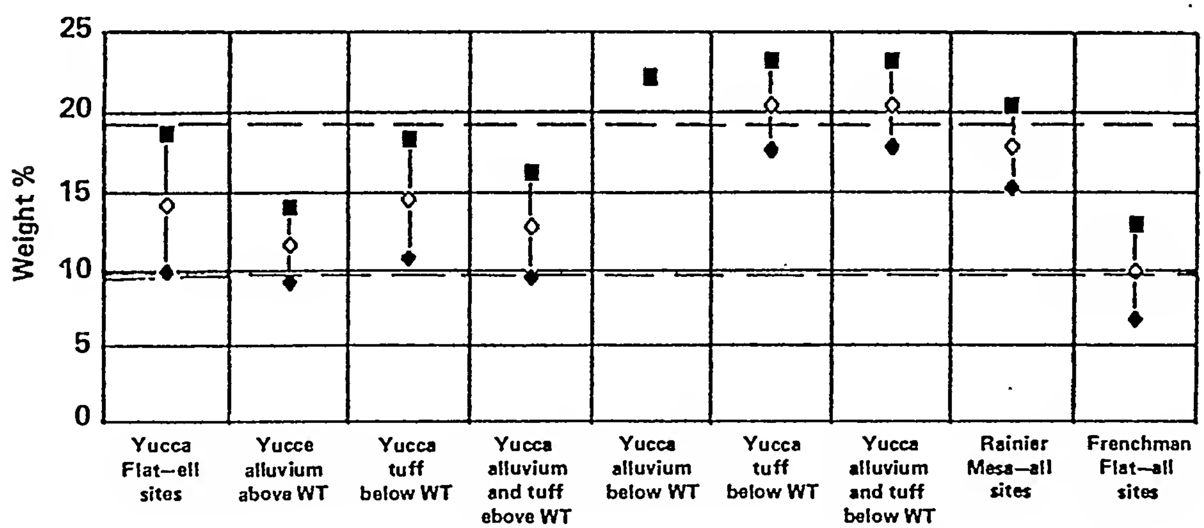
Working point density—Yucca Flat and Rainier Mesa. Dashed lines represent the range of ±1 standard deviation for all Yucca Flat sites.

from Howard, 1985

Figure 2



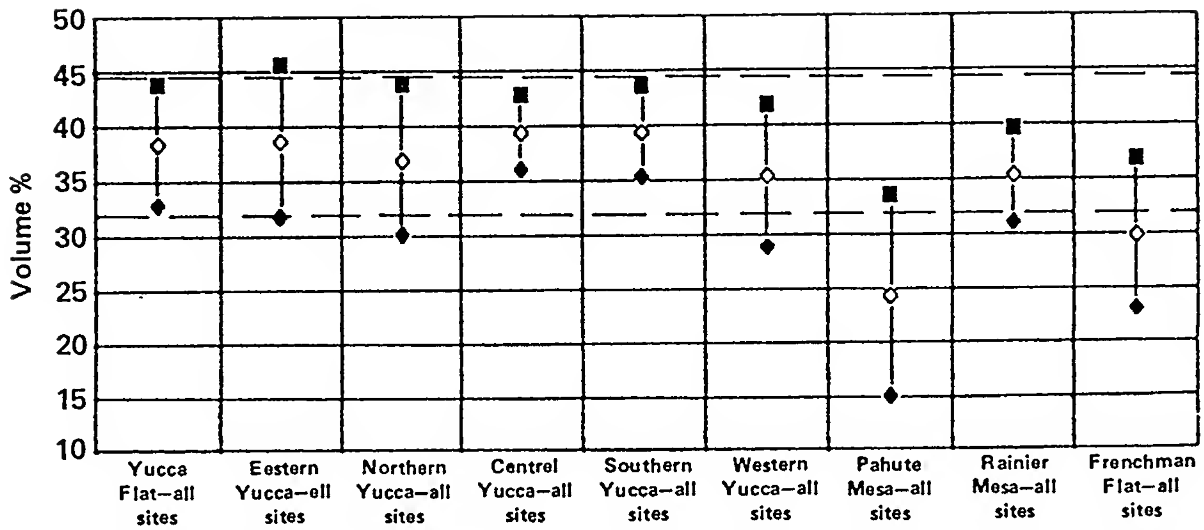
Working point water content—all areas. Dashed lines represent the range of ±1 standard deviation for all Yucca Flat sites.



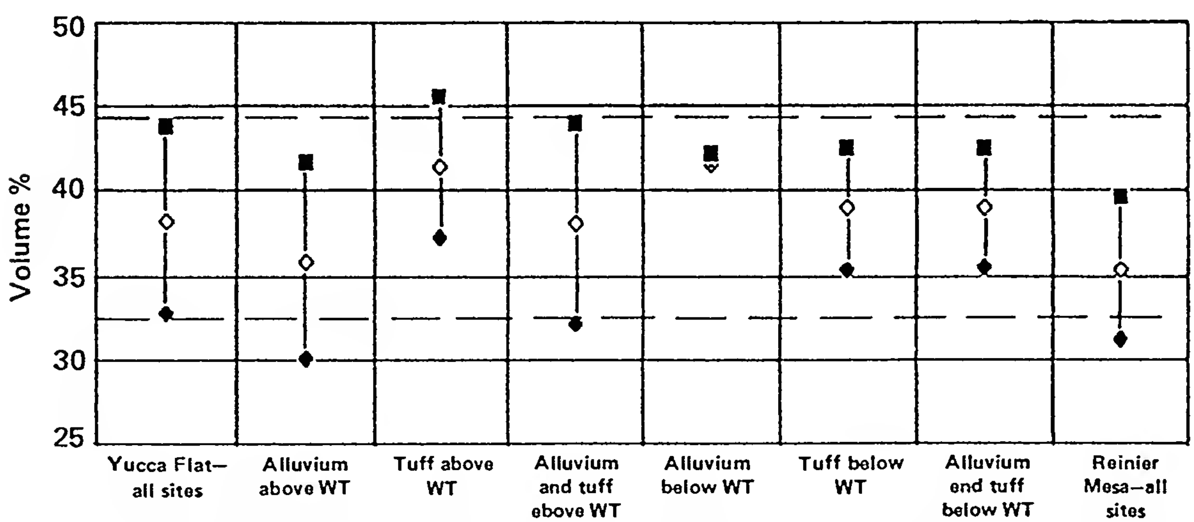
Working point water content—Yucca Flat, Rainier Mesa, and Frenchman Flat. Dashed lines represent the range of ±1 standard deviation for all Yucca Flat sites.

from Howard, 1985

Figure 3



Working point porosity—all areas. Dashed lines represent the range of ±1 standard deviation for all Yucca Flat sites.



Working point porosity—Yucca Flat and Rainier Mesa. Dashed lines represent the range of ±1 standard deviation for all Yucca Flat sites.

from Howard, 1985

Table 1

Mean Zonal Properties
and
Standard Deviations of the Zonal Properties about the Mean

Unit	Grain Density Mg/m^3	Bulk Density Mg/m^3	Water Content Wt %	Porosity Vol %	Saturation %	Gas Porosity Vol %
QTAL	2.59±0.04	1.90±0.12	11.2±3.9	34.6±4.4	59.7±9.7	14.0±4.2
QTMA	2.63±0.05	1.96±0.10	9.9±2.3	32.8±3.8	58.5±8.8	13.5±3.1
QTTA	2.56±0.05	1.79±0.11	13.4±3.1	39.5±4.5	60.2±10.6	15.7±4.8
TMA	2.53±0.06	1.67±0.12	15.8±4.6	44.2±4.5	59.9±17.0	17.7±7.8
TMB	2.55±0.08	1.68±0.10	16.3±4.6	45.2±3.9	60.3±15.0	17.8±6.6
TMR	2.51±0.04	1.74±0.10	14.2±4.0	40.3±5.0	60.7±12.3	15.8±5.3
TP	2.49±0.06	1.63±0.11	15.6±2.1	44.3±4.6	57.8±9.6	18.9±5.5
TBG	2.45±0.07	1.39±0.18	19.8±5.3	54.3±6.7	50.9±15.1	26.9±9.2
TTB	2.49±0.05	1.66±0.10	15.8±2.8	43.8±4.1	60.0±10.9	17.6±5.4
TTS	2.47±0.05	1.60±0.14	18.4±3.3	47.1±5.6	63.2±12.8	17.7±7.7
TB	2.57±0.06	1.73±0.12	14.5±0.9	42.2±2.9	59.9±9.3	17.1±5.1
TF	2.56±0.05	1.75±0.05	14.6±1.2	41.4±1.9	61.5±4.1	15.9±1.7
TRV	2.59±0.02	1.60±0.14	12.2±4.3	45.8±6.4	41.5±10.2	26.6±4.7
TU	2.54±0.08	1.74±0.09	15.4±1.0	41.9±2.0	64.0±6.5	15.1±3.1

Stratigraphic Nomenclature

QTAL	=	Alluvium
QTMA	=	Mixed alluvium
QTTA	=	Basal tuffaceous alluvium
TMA	=	Ammonia Tanks Member
TMB	=	Bedded Ammonia Tanks
TMR	=	Rainier Mesa Member
TP	=	Paintbrush Tuff
TBG	=	Grouse Canyon Member
TTB	=	Tunnel Beds
TTS	=	Tub Spring Member
TB	=	Bedded tuff
TF	=	Fraction Tuff
TRV	=	Red Rock Valley Tuff
TU	=	Undifferentiated tuff

from Burkhard, 1989

quite different event phenomena depending upon the DOB selected. A real difficulty is that there are surprisingly few instances where the site itself has been blamed in a containment failure, with the possible exception of CO_2 content. The great majority of releases resulted from man-made weaknesses, usually cables or stemming. Even the cases of a release resulting from failure of the geologic medium were usually aggravated by a poor choice of WP (or DOB) for the yield involved, and the site was not unequivocally "bad". This simply emphasizes the necessity of considering multiple factors in the containment analysis.

Table 2 lists the events where the geologic setting was generally agreed to have been a contributor to a release; all dynamic ventings are included, and seeps are included where there was a lesson learned. The table is restricted to vertical events that were conducted after the end of the moratorium in September 1961 (Schoengold, et al., op. cit.). Pre-moratorium events are not included because they were generally poorly documented and were conducted with little concern for containment. The containment knowledge from those tests usually related to scaling, and the available geologic data were too sparse to characterize the site. Two events conducted in high carbonate content rock are listed Table 2 even though they did not have a detected release.

In addition to the events in Table 2, there is a group of events on Pahute Mesa, e.g., KAPPELLI, TIERRA, and LABQUARK, that had very late-time, low-level releases of noble gases. The releases are related to passage of low-pressure weather systems, and, while detectable with special sampling equipment, are not considered breaches of successful containment; the CEP has defined a seepage-type release of noble gases after H+24 hours as being within the definition of successful containment. This type of seep is treated in the NTS Environmental Impact Statement as a possible consequence of testing, not an anomalous occurrence. These releases, commonly referred to as "breathing", have only been observed where Timber Mountain Tuffs are the surface rock type; these rocks are known to have long vertical fractures, usually cooling joints, and relatively low porosity. It is not uncommon to detect a specific fracture or small set of fractures, not a fault, at the surface where the activity is concentrated. The known cases of this type are few and relatively recent, since 1984 when the gas sampling systems needed to detect such low-level seeps were deployed on Pahute Mesa. Earlier instances, particularly following the high-yield test campaign in 1976, may have been undetected. In any event, there is no hazard associated with these occurrences. While one must keep these events in mind during the siting process, such events are not, by definition, containment failures, and do not preclude siting a new location nearby.

RULES-OF-THUMB

We will now review the guidelines used in siting and containment evaluation and, where appropriate, discuss the rationale and weight given to each. The items are in no particular order; that is intentional, not simply the lack of organization. There is often overlap, as will be obvious, and there is also danger in implying that some things are more important than others. While it is certainly true that some topics are more critical than others, the more important factors vary with the site and the specific parameters of the proposed test. Prudent and thoughtful siting requires that all relevant topics are considered, and that if something is disregarded, it is done so by intent rather than by accident. In the same vein, one must avoid merely going down a check list, since the temptation to forget interactions is too great.

GEOLOGIC RELEASES

EVENT	DATE	HOLE	YIELD	DOB	RELEASE	COMMENTS
HARD HAT	2/15/62	U15a	5.7 kt	287 m	Yes - small	In granite, late-time seep near SGZ
EEL	5/19/62	U9m	<20 kt	217 m	1.9 x E+6	Only dynamic vent via sattelite hole
						for a vertical event
WICHITA	7/27/62	U9y	<20 kt	150 m	7.6 x E+2	Vent through fissure ~50' N of SGZ,
						low SDOB
BANDICOOT	10/19/62	U3bj	<20 kt	241 m	3.0 x E+6	Vent through fissure near SGZ
PIKE	3/13/64	U3cy	<20 kt	114 m	1.2 x E+5	Vent through fissure at +10 seconds
FADE	6/25/64	U9be	<20 kt	205 m	3.5 x E+1	Seep starting at +2.75 hr. Only
						post-collapse seep for event in tuff
HANDCAR	11/5/64	U10b	12 kt	403 m	6.4 x E+1	In carbonate rock, late-time seep
DRILL	12/5/64	U2ai	3.4 kt	188 m	6.1 x E+4	Seep from crater, 10.5% CO_2 alluv.
PINSTRIPE	4/25/66	U11b	<20 kt	295 m	2.1 x E+5	Vent from fissure near SGZ related
						to fault seen in hole, LOS part of path
PILEDRIVER	6/2/66	U15a	62 kt	462 m	3.7 x E+4	In granite, noble gas seep near SGZ
						starting at about H+12 hr.
KANKAKEE	6/15/66	U10p	20-200 kt	454 m	0	In dolomite, no detected release
NASH	1/19/67	U2ce	20-200 kt	365 m	6.9 x E+4	In dolomite, seep from crater
						started at H+9.25 hr.
BOURBON	1/20/67	U7n	20-200 kt	559 m	0	In dolomite, 23.3% CO_2, no release
SCROLL	4/23/68	U19n	<20 kt	224 m	1.8 x E+4	Satellite hole seep, stemming failure,
						only release on Pahute Mesa
CREAM	12/16/70	U9 X-29	<20 kt	294 m	66	Seep attributed to cables could be
						geologically related, poorly documented
BANEBERRY	12/18/90	U8d	10 kt	278 m	6.7 x E+6	Vent from fissure related to fault
DIAGONAL LINE	11/24/71	U11g	<20 kt	264 m	6.8 x E+3	Seep from crater, 7.1% CO_2
AGRINI	3/31/84	U2ev	<20 kt	320 m	690	Seep from anomalous crater, deep and
						offset from SGZ

Table 2

(1) **A new emplacement hole must be 4 hole depths away from an existing, open emplacement hole, based on the deeper hole.**

This is an operational, rather than a containment, requirement and is intended to avoid hole damage. The four hole-depth figure is a conservative way of approximating four DOB and is intended to keep the adjacent hole outside the maximum-credible spall zone of a test. The separation works well on Pahute Mesa where holes are, in general, somewhat more stable, since there is seldom alluvium to drill through on the Mesa. The spacing is preferably greater on Yucca Flat, but since the holes are generally not so deep as on Pahute Mesa, the absolute distances are generally smaller.

(2) **Containment planning is based on Maximum-Credible Yield.**

There are several yields associated with any given device or test. Pre-event containment analysis is usually based on *Maximum-Credible Underground Yield*, which is defined as "Preshot estimated yield, assuming that the test device will perform at maximum credible efficiency, plus estimated yield enhancement from radiation capture in the surrounding medium." This is the yield used to set the DOB for an event. There are other yields, where yield is defined as "The total energy released by detonation of a nuclear weapon." The *Design Yield* is the "Preshot estimate of the most probable test device yield", where *Test Device Yield* is "The yield produced by nuclear reactions within the test device as determined after a shot. Does not include yield enhancement from radiation capture in the surrounding medium." For containment purposes we are interested in the total energy deposited in the medium, hence, when studying experience, containment analyses should use the *Event Yield*, defined as "The sum of the device yield and yield enhancement by energy release from radiation capture in the surrounding medium. This yield is a best estimate made on the basis of radiochemistry, seismic, and other data, and is subject to change as new analyses are conducted." These definitions are from the DOE Office of Classification and have been adopted for use in the LLNL Containment Data Base. Note that containment most often uses underground yields, and that the definition of design yield excludes capture yield. Containment should specify and use "underground design yield" which includes capture yield. The capture yield comes from neutron capture in the materials adjacent to the device and is normally a few percent of the Test Device Yield. On rare occasions the capture yield can be an enhancement as high as about 20% depending upon the details of the device and surrounding materials. Although the DOB is set based on maximum-credible yield, the (underground) design yield is, of course, important, since it is the best estimate of what will actually happen and what the actual threat to containment will be. Design yield must be kept in mind especially when formulating the stemming plan, particularly if there is a large difference between design and maximum-credible yields, as sometimes happens. Also of interest is the distinction between fission and fusion yield. As a source of radionuclides for a potential release, fusion yield does not, to first approximation, generate radioactive debris. The potential release inventory is based on fission-plus-capture, and a release fraction using curies released should use that value as the source term. Fission-plus-Capture is reported to DOE prior to each event, and that value is used in exposure and fallout predictions at the time of test execution.

At LLNL a classified Device Status Sheet is published by the Nuclear Design Office that gives the necessary data to determine each of the various yields mentioned above. On rare occasions the difference between design and maximum-credible yield may be so small, much less than 10%, as to raise a question whether the values are reasonable. The containment issue is whether the maximum-credible yield stated is really the maximum if the design yield is so close

and if yield measurements are, in general, ±10%. The device designers should be consulted; the general answer is that there is high confidence in a well tested design, although in one case the author brought to light an error on the part of the designer by questioning the values given. That was different from a simple typographical or addition error, which is regrettably not too uncommon. The lesson is that the Status Sheet must be read; don't just copy the bottom line.

(3) The minimum DOB for any nuclear test is 600 feet (183 m); the minimum DOB for a "complex" test is 200 m.

The minimum DOB is independent of yield and was determined by examination of the incidence of releases as a function of depth. It was found that for depths less than about 600 ft, the likelihood of some seepage was considerably increased regardless of yield. The amount of porous overburden needed for containment was, therefore, set at 600 ft. The "complex" depth of 200 m, or 17 m deeper, was developed for long LOS canisters, in particular Mechanical PINEX events, where it was assumed that the LOS could effectively bypass some of the reservoir capacity of the overburden or that it could act to move the center-of-energy upward. The minimum dates from the late 1960s and has been lowered on a few occasions when the yield involved was extremely low, a maximum credible yield of no more than a few tens of tons and a design yield of not more than a few tons of nuclear yield, preferably a design yield of zero. As could be seen in Table 2, no such minimum existed during the early years of underground testing.

(4) The Scaled-Depth-of-Burial (SDOB) is nominally 122 m/kt$^{1/3}$.

The SDOB of 122 m/kt$^{1/3}$, or 400 ft/kt$^{1/3}$, was developed as a result of adjustments made to general practice following BANEBERRY. In siting contexts the SDOB is based on the maximum-credible yield. Prior to BANEBERRY the "prescribed SDOB" was 350 ft/kt$^{1/3}$ (Germain and Kahn, 1968). The change was made for conservatism following BANEBERRY, and is interesting in that BANEBERRY had a SDOB of 129 m/kt$^{1/3}$. The 350 value came from experience and analysis of underground tests as well as cratering tests, and was considered to be conservative to prevent a prompt dynamic failure. There were several successfully-contained tests in the 1960s with SDOBs in the range of 300 ft/ kt$^{1/3}$ (91 m/kt$^{1/3}$), hence 350 was deemed conservative.

The 122 m/kt$^{1/3}$ scaling stops at 3.4 kt, where the scaling law intersects the 183 m minimum DOB. Below that yield DOBs are no longer set by reference to scaling. In practice the 122 value is used for Yucca Flat events in the 10 kt range. Pahute Mesa events in the >100 kt range are generally buried with a SDOB in the 115-118 range; a few Mesa events have been presented to the CEP and approved with a SDOB of 111 m/kt$^{1/3}$ at maximum-credible yield. Events with yields less than about 10 kt are commonly sited with a SDOB of 125-130 at maximum-credible yield. Events in the upper yield range for Yucca Flat, about 50-100 kt, are generally buried with a SDOB of about 117-120 m/kt$^{1/3}$. The commonest reason for pushing the lower limit in SDOB is to stay above the standing water level in a given hole or to be able to use a hole without additional drilling or remedial work.

The lowest SDOB ever presented to the CEP was 110 m/kt$^{1/3}$ at maximum-credible yield. Prior to BANEBERRY and the formation of the CEP in 1971, lower SDOBs were not unusual, but the political climate post-BANEBERRY dictated that additional conservatism was not just prudent but essential. The SDOB is a primary parameter in containment evaluation.

(5) The cavity radius (Rc) is estimated to be (K x $W^{1/3}$)÷(Rho x h)$^{1/4}$, where W is the yield in kt, Rho is the overburden density in Mg/m^3, h is the DOB in metres, and K is a constant, nominally 70.2; the resulting cavity radius is in metres.

The cavity radius is only approximated by the above formula and has 1-sigma error bars of about 10%. The constant is normalized to cavity radius data obtained from post-shot drilling in the lower hemisphere of the cavity. There is some evidence of areas that have a systematically different K value; Pahute Mesa tends to be a bit low, and some regions in southern Yucca Flat seem to be a little high. There have been other cavity radius formulas proposed, but this is the one that has "stuck". The only real improvement over the simple formula above involves strength related parameters that are unknown, at least before siting an event. We have no proven way of measuring *in situ* strength properties. Terhune and Glenn (1977) developed a relationship between shear strength and the measured cavity radius. The problem with this is that it can not be used as a predictor, and it depends on taking the fourth-power of a measurement that has errors of about 15% (Hudson and Smith, 1983). The cavity radius is used as input in several other guidelines.

(6) The minimum Radii-of-Burial (ROB), where ROB is DOB÷Rc, is 7.6.

The ROB has been used for a number of years as an alternate to SDOB. It was championed by Charles Browne of LANL in 1967 and was primarily a way to systematize the use of lower SDOB for higher yields, as mentioned in #4, above. The ROB is, in effect, $W^{4/15}$ rather than $W^{1/3}$ scaling, if one uses the Rc formula just preceding. This lowers slightly the yield dependence in scaling and makes a significant difference for yields of several hundred kilotons or more. If one sets $W^{1/3}$ and $W^{4/15}$ scaling to give the same DOB at 10 kt, the alternate scaling rule is 142.2 $W^{4/15}$. At 1 Mt the cube-root scaling gives a DOB of 1220 m while the alternate gives 897 m, a significant difference, especially operationally. The ROB parameter is routinely reported in the CEP Data Synopsis Sheet, but is generally given less weight than SDOB, especially since the 150 kt limit of the Threshold Test Ban Treaty of 1976. The present value of 7.6 as a minimum was proposed by LANL primarily because the measured ROB for BANEBERRY was 7.5; in fact, a value for ROB near 9 is generally used for design purposes.

(7) Prompt failure will occur at a free surface closer than about 60-70 m/kt$^{1/3}$.

This distance is material dependent and applies to a free surface large enough to generate a rarefaction; a small feature, small with respect to the magnitude of ground motion involved, such as a small diameter hole, is not a concern in this context. This distance is on the edge of incipient cratering for a vertical emplacement and tunnel failure for a mined feature. SULKY, a "cratering " test, was at a SDOB of 60.1 m/kt$^{1/3}$; the result was a mound of broken rock, rather than a crater; the inverted crater was dubbed a "retarc". The end-of-stemming in DNA tunnel tests, that is, the distance where cementing terminates and open tunnel exists at shot time, is normally at a scaled range of about 75 m/kt$^{1/3}$, and occasional tunnel damage is still observed. Again, the distance is quite medium dependent, being a function of both the shock attenuation characteristics and the material strength at the surface in question.

(8) The minimum-impeded-path to the surface is 7.6 cavity radii.

If the shortest path to the surface is straight up, or wherever the nearest free-surface to the WP is, if the surface terrain is uneven, this is merely a restatement of Guideline #6. The intent is to say that no path through any route, namely open drill holes , adjacent chimneys, etc., will be

less than 7.6 cavity radii. This is not a strict rule, since there is successful experience with an impeded path of slightly less than five cavity radii. LANL tends to adhere to this somewhat more strictly than LLNL. The nature of the path is obviously of importance. A large open hole going directly upward for 2.4 cavity radii from a device buried at 10 Rc is clearly of more concern than a small diameter drill hole that goes to the surface but approaches to no less than 7.6 Rc to a WP. The minimum-impeded-path is generally used as a flag, but the nature of the path is of more importance than the simple number of cavity radii, and each pathway that is less than a DOB in length warrants consideration. It is generally assumed that a path to the atmosphere that exceeds 1 DOB is less of a threat than going straight to the surface. While one could envision a setting where that might not be true, for example a tunnel test, from a practical standpoint it is a suitable assumption for vertical events. If there is a pathway that is a potential hazard, the path can be remediated by cementing an open post-shot hole, for example, by moving the proposed hole, or by moving the WP in an existing hole. In some cases it is prudent to cement a hole even though it is far enough away from a proposed site because the hole may be close enough that caving could be caused by an event, and the hole could not be properly plugged for a future site that is closer.

(9) The WP should be no less than 2 Rc from the Paleozoic rocks.

The Paleozoic rocks (Pz) generally present a significant mismatch in material properties, being much harder than the overlying rocks. As discussed in the previous section on Geologic Structure, materials that alter the spherical symmetry of the ground motion can reduce the residual stress. A WP in relatively weak tuffaceous rocks near the Pz surface could generate motion that hits the hard surface and is deflected outward along the interface. Rebound of such motion will not be radially inward toward the cavity, and the stress field will be degraded. The hard surface also serves as a reflector, and enhanced upward motion may be generated, also complicating rebound phenomena. There is often a layer of Paleocolluvium at the tuff/Pz contact that has high clay content and is quite weak and can serve as a lubricant for motion along the surface. There have been tests closer than 2Rc from the Pz surface with no deleterious effects when the Pz rocks are generally below the WP, such as COTTAGE in U8j. If the Pz rocks have been brought near the WP by faulting, and they are to the side or even above the WP within a few cavity radii, the ground motion and resulting stress field can be strongly altered. The prime example of this circumstance is BANEBERRY. It is possible to safely execute a test with the Pz surface closer than 2Rc, and there are several examples, but maintaining the prescribed separation simplifies containment analysis and is useful in siting studies. A proposed test much less than 2Rc from a major mismatch in materials will almost certainly require computational modeling for adequate evaluation. The need to locate the Pz surface is one of the major reasons why emplacement holes are overdrilled or exploratory holes are requested, especially in an area where there is not good structural control, and is also a major impetus for the extensive gravity modeling that has been done of the NTS.

(10) High contents of swelling clay should be avoided.

Smectite clays, primarily montmorillonite, are widespread alteration products found in the Tertiary volcanics and, commonly, near the tuff/Pz contact. The montmorillonite is generally found in relatively small percentages or in quite restricted areas when high clay contents are found. Clay content determination is a primary reason for x-ray analyses of rock samples. High clay content presents a drilling problem, since it can "ball-up" or gum-up the drill bit, although locations where this has occurred are unusual, generally found in Paleocolluvium at the tuff/Pz contact. The containment concern with swelling clay stems from BANEBERRY, where the WP

was in a large "pod" of altered tuff with the montmorillonite content >60% throughout a large volume; much of the material had 80% swelling clay, and some samples exceeded 90%. Smectite minerals, of which montmorillonite is an example, swell when in contact with water and the resulting material has very low strength; the shear strength for saturated samples of montmorillonite is essentially zero.

From a practical standpoint, a site with large volumes of highly altered material are not likely to be encountered. The BANEBERRY site was probably unique. Since relatively modest amounts of montmorillonite are reasonably widespread, the more common question is "when do we start getting concerned?" The most common place to find high clay contents is near the tuff/Pz contact, as mentioned earlier. Since we tend to avoid the Pz rocks, such clay occurrences are not often a concern. Some alteration is relatively common in some of the tuff units. Alteration occurs in vitric materials when in contact with ground water. The real concern is with very weak material, not with clay content, *per se.* Montmorillonite is generally not evenly distributed, but tends to occur in small vesicles of high clay content distributed in the rock matrix. A sample with an average content of, say, 50% clay can still be relatively strong depending on the nature of the unaltered matrix and the amount of water present. The CEP has suggested that any detected montmorillonite content >20% should be reported. Individual samples >20% are not uncommon, but, unless there is a fairly large volume of material with >40% or so, clay is not a major concern. There are instances where computational modeling of a material interface as having essentially zero strength has influenced calculated phenomena, but, with the sole exception of BANEBERRY, no site has ever had clay implicated in a containment failure. Even on BANEBERRY the clay was only one of several factors implicated, not the cause of the failure (Terhune et al., 1978). Once the lesson was learned on BANEBERRY, similar sites are easy to recognize and avoid. Additionally, a relatively high water content is required along with the clay to significantly reduce the strength; water was one of the other factors, along with the fault, cited as a cause of the BANEBERRY venting.

(11) A WP CO_2 content >4.5 wt% is unsafe.

The question of noncondensible gas generation has received considerable attention over the years. The prime example of a CO_2 driven release is the NASH event, which was fired in dolomite with about 40 wt% CO_2. The release was characterized as late starting (nearly 10 hours post-shot), long duration (about 40 hours), and diffuse in release location. The hazard involved in noncondensible gas generation is complex. At the least, the parameters of concern are the CO_2 content of the medium, the yield, which determines how much of the rock is decomposed, the DOB, and the gas storage and transport properties of the overburden. For siting purposes we generally assume that CO_2 in tuff is negligible. Individual samples, usually from a weathered horizon may contain some CO_2, but the bulk CO_2 content of tuff is virtually always <0.5 wt%. While four tests were conducted in carbonate rocks in the past, it is most unlikely that such a test would be conducted today, even though such tests have been proposed occasionally, usually as seismic experiments. From a practical standpoint, the CO_2 question applies only to sites with the WP in alluvium.

The amount of gas generated is a function of the CO_2 content multiplied by the yield of the explosive, which is the source of energy to decompose the carbonate minerals and produce CO_2. This fact, of course, indicates that our rule-of-thumb is incomplete, since 5 kt in a 1% CO_2 medium will, to a first approximation, generate as much gas as a 1 kt explosion in a 5% CO_2 medium. The 5 kt test would, however, be buried more deeply, and hence have a larger "sponge "

above it. The 4.5% value is from an extensive study of event release data as a function of CO_2 content done by Howard (1983) who concluded that a CO_2 <4.5 wt% was clearly within good experience. It was not a treatment of gas transport and, of necessity, averaged out other parameters. The value of 4.5%, while supported by history, is generally not used as a guideline, since it does not have conservatism built-in; it simply summarizes experience for "standard" events. In practice a CO_2 content of 3-4 wt% tends to be the range where concern surfaces, and events with >4 wt% have seldom been proposed in recent years. In general, events seen in the last few years with alluvium WPs have had very low yields, and the argument that the absolute quantity of gas generated is very small has been invoked. The preference is for WPs in tuff, thereby avoiding the entire question.

There have been four events conducted in, more-or-less, pure carbonate rock, NASH (40 wt% CO_2 at 365 m depth), KANKAKEE (39 wt % CO_2 at 454 m depth), HANDCAR (47 wt% CO_2 at 403 m depth), and BOURBON (17 wt% CO_2 at 560 m depth); releases were detected from NASH and HANDCAR, but not from the other two.

Keller et al. in 1987, in an effort to systematically study the question as a gas-transport problem and explain the apparent anomalies just mentioned, have developed an expression for time-of-arrival (TOA) at the surface:

$$\text{TOA}=[C \times \varepsilon^2 \times (\text{DOB})^3] \div (K \times f \times M \times W^{1/3})$$

or

$$\text{TOA}=[C \times \varepsilon^2 \times W^{2/3} \times (\text{SDOB})^3] \div (K \times f \times M),$$

where TOA is in hours, C=0.0155, ε is the porosity, W is in kt, DOB is in metres, SDOB is in m/kt$^{1/3}$, K is formation permeability in darcys, f is wt. fraction CO_2 in the medium, and M is the mass of rock heated to drive off CO_2 in tonnes/kt of yield. The treatment assumes flow in a chimney, that is, it starts after collapse, which is one of the flaws in the analysis. While there are some assumptions that are open to question, the analysis does take into account the parameters that intuitively seem important. The difficulty in using the expression lies primarily in the difficulty in obtaining measurements of the permeability and, to some extent, the porosity, and knowing how to determine an effective value when actual values vary with location. The expressions do indicate that increased depth or porosity is good and that increased gas generation or permeability is bad.

(12) CO_2 is generated from a mass of 3300 tonnes/kt of yield.

Various estimates based on different forms of analysis have been used to estimate the value of "M" in the preceding section. The value of 3300 tonnes/kt is derived from a series of 1-D hydrodynamic calculations performed by Butkovich (Butkovich and Lewis, 1973) that estimated the energy deposition and mass heated sufficiently to decompose carbonate rock. The energy deposition is density dependent, and a more exact expression (Dreiling, 1980) is:

$$M= 5018 - 218\rho - 373\rho^2$$

where ρ is the bulk density. M=3300 at a density of 1.87 Mg/m^3, which is a reasonable average density at the NTS. This expression is not commonly used because 3300 is so much more easily remembered. The LLNL uses 3300 for M to calculate the averaging interval for CO_2 samples in its site characterization. A difficulty is that the decomposition is a rate process, hence the decomposition is dependent on the thermal history as well as the specific carbonate minerals present. On occasion concern has been voiced over a region of high carbonate rocks that is

stratigraphically just above the zone that is promptly heated enough to evolve gas but is close enough that, on cavity collapse, it could fall into the warm cavity region and be further heated sufficiently to begin releasing CO_2. There has never been any clear evidence that this has occurred, but the configuration should be avoided if possible. Gas sample analysis by Nuclear Chemistry has given ambiguous results as far as supporting the currently used value of "M", but the data have not been sufficient to support an alternative value. The data do imply that the processes, at least sometimes, are more complex than the extant model describes.

(13) Other sources of noncondensible gas may be important.

There can be noncondensible H_2 generated by the oxidation of metals in the device and diagnostics canisters. Several investigators have looked at this as a source of noncondensible gas, most notably Gaffney (1983) at LANL. Since the amount of material is fixed and does not increase with yield, as does available CO_2, noncondensible gas related to oxidation of canister materials by the steam in the cavity decreases with yield. Additionally, magnetite that is often used as stemming material around canisters mitigates H_2 formation. In most cases the thermodynamic conditions favor oxidation of iron but not of lead and aluminum, the other major metallic components of canisters. In some cases organic materials, such as polyethylene and B_4C, may also contribute appreciable noncondensible gas, but in most cases the iron is the primary contributor. If expressed as CO_2 equivalent, $\%CO_2 = 0.024$(tonnes of Fe / yield in kt). Hence, 10 tonnes of Fe in a canister could generate the equivalent of 0.24% CO_2 at 1 kt, and 2.4% at 0.1 kt. Even very large canisters can produce only trivial amounts of gas at yields above a few kilotons. Metal generated gas may become significant at very low yields, but in such cases the entire assembly probably will not be involved, and the absolute amount of gas generated will generally be small even if it is equivalent to a significant CO_2 content.

(14) The shock wave promptly vaporizes rock to 2 m/kt$^{1/3}$.

The energy deposited in the surrounding rock as a result of the explosion promptly vaporizes a mass of rock; the vaporization radius is 2 m/kt$^{1/3}$. Butkovich studied energy deposition in the material surrounding an explosion, and for silicate rocks determined the mass vaporized is about 70 tonnes/kt (Butkovich and Lewis, op. cit.). The value is rock type dependent, but virtually all WP materials at the NTS can be reasonably approximated as silicate rocks. The vaporization radius in metres, Rv, is then simply:

$$Rv = (16.7/\rho)^{1/3}$$

where ρ is the bulk density. For a density of 2.0, Rv = 2.0 m. For a density of 1.6, Rv increases to 2.19 m, and a density of 2.3 gives a radius of 1.94 m. For a representative range of densities, the guideline is good to better than 10%. Since Rv is a function of the cube-root of the energy of vaporization, the rule-of-thumb holds reasonably well. For low yields one must use the estimation with some care, since in most cases the drill hole has a radius of about 1.2 m and the canister hardware provides a severe departure from the assumptions involved. The guideline assumes spherical symmetry, which does not hold when man-made features are of the same scale as the estimated Rv.

(15) The melt radius, Rm, is 4 m/kt$^{1/3}$.

The prompt melting obviously extends beyond the vaporized volume. The energy deposition is dependent on the material through which the shock propagates. As mentioned

earlier, Butkovich, assuming silicate rock, fit energy deposition results from 1-D calculations, and found that the temperature rise is dependent on density. The basic reason is that a lower-density, more-compressible rock uses more PΔV work, resulting in higher temperatures at shorter distances. The amount of melt was estimated to range from about 1000 tonnes/kt for high-density, low-porosity rocks to nearly 2500 tonnes/kt for low-density, very-compressible rocks. These values correspond to Rm ranging from 4.3-5.8 m/kt$^{1/3}$. **A better rule of thumb is a value of 5 m/kt$^{1/3}$**, and is probably good to ±15% for the majority of rocks at NTS; some adjustment might be appropriate if one is dealing with a material near the extremes of NTS experience. As with Rv, the estimate is suspect for small values due to lack of symmetry. The 4 m/kt$^{1/3}$ value is in use partly because earlier estimates, 700 tonnes/kt, were lower than the values give above and partly because it is easy to remember, being double Rv. The 700 t value was used recently in a study of gas-tracer detection in cavity gas; the results of that study were consistent with a value of 6.0 ± 0.5 m/kt$^{1/3}$ for the radius of "incorporation range", the distance over which material will intermingle with the cavity gas. It seems reasonable that distance might be slightly in excess of the actual melt radius, since later time phenomena are included.

(16) High water content is not, *per se*, a concern.

There are reasons to be concerned about the water content near the WP and at the site in general. The first concern is simply, "Is there standing water?" Drilling below the water table generally runs a higher risk of hole caving. Emplacement of hardware in a wet hole obviously requires special care with attendant cost penalties, particularly with electrical systems. There was at one time a major concern over emplacement of stemming below the surface of standing water (standing water level or SWL), as well as concern of how saturated stemming material would respond to ground shock. Drill holes can, of course, be cased, but that is expensive and decreases the effective diameter of the hole. Water leaks in casings and liners (a partial hole casing) has also been a problem. The ex-USSR Test Site at Semipalatinsk has a SWL very near the surface; they developed procedures for stemming holes that were almost completely water filled. Rather than simply pouring in material, the Soviets had plans than accounted for material sinking through water for large distances and settling until a desired density was reached, and they had to deal with water coming out of the hole at SGZ as it was displaced by stemming.

Water in the medium provides the great majority of the material comprising the cavity gas; the rock materials have, in general, condensed by the end of cavity growth. The water content affects the cavity pressure history; pressure decay is slower with more water. The steam is also condensable; as the cavity cools sufficiently, or is quenched by material falling in, the majority of the working fluid condenses, and the driving pressure drops considerably.

While water certainly affects event phenomena (Butkovich, 1971), there is no real evidence that water, in itself, is critical. Water content is a parameter that is used to calculate saturation, a property that strongly influences shock coupling and attenuation. Water was a primary suspect in BANEBERRY because the water content, 25%, was the second highest ever measured for a WP region at the NTS. The highest, 27.4%, on ROUSANNE in U4p was found subsequent to BANEBERRY and did not lead to any untoward behavior. It is generally agreed that the high water content at BANEBERRY led to a problem because it was teamed with a very high swelling clay content making a large mass of very weak material. Saturation does increase ground motion, but a large number of events have been fired below the water table with no release ever attributed to that fact. Water content, then, is a critical parameter in the

understanding of a site, but high water, *per se,* is not a problem; it is a flag that other factors, clay, for example, must be investigated.

(17) A WP should be at least 10 m above any standing water in a drill hole.

This is a guideline to be used when determining the greatest DOB, and therefore the highest yield, suitable for a given drill hole, and assumes adequate geologic control (see #19, below). The guideline implicitly takes into account the length of the device canister below the WP, the uncertainty in the water level measurement (usually small), and the possibility that minor sloughing during emplacement could cause a transient rise in water level. In practice the hole is usually pre-stemmed to about the same level so that the hole is dry during emplacement of the canister assembly. The final distance above the standing water must be agreed to with engineering personnel to make sure there are no special circumstances, such as an unusually long device canister.

(18) A WP should be at least 5 m above the bottom of a dry hole.

This guideline is analogous to the preceding one, but is for dry holes. In the case of dry material, fill in the hole could have a very non-level surface and could have some bridging, so the tag has a greater uncertainty than does a tag in a wet hole. Some space is also necessary in the event of minor sloughing and hole fill during emplacement. A hydrodynamic yield measurement pursuant to the TTBT adds an additional constraint. The real judgment is whether there are sufficient geologic data bearing on material below the bottom of the hole to place the WP so near the bottom, which leads to the next guideline.

(19) Adequate control on material properties to at least 1 Rc below the WP is required, and adequate structural control is required for at least 2 Rc below the WP.

The CEP standards for data presentation (W. S. Twenhofel et al., Final Data Needs Subcommittee Report, 1983) request material properties averages over the interval WP±Rc for all parameters except CO_2, which is averaged, as previously discussed, to a range encompassing 3300 tonnes/kt, which is generally about 0.6 Rc. *Note that the guideline is a working generalization of the CEP report; it is not an actual CEP rule.* The averaging interval is based on the maximum-credible yield cavity radius, but the CEP has requested that it be informed if there is a significant fluctuation in properties as the interval is changed, since this is indicative of a heterogeneous site.

The main reason for the 2Rc structural control requirement was discussed in Item #9, above, and has to do with adequate knowledge of material properties discontinuities. If one knows the location of the Pz surface or of a significant fault from tags in adjacent holes, or from seismic or gravity interpretations (often a combination) it is not necessary that the given hole provide a tag of the Pz rocks. The existing stratigraphic data from the hole must, however, be consistent with the regional information if data to 2Rc below the WP are not available. If regional data are sparse, more conservatism must be included in the geologic interpretation and in the choice of WP depth. If a Pz tag was made in the hole, and if a nearby fault that would bring the Pz rocks closer to the WP can be ruled out, the WP can actually be somewhat less than 2Rc above the Pz surface. It is the responsibility of the site geologist and other resource persons to determine if adequate control is available to provide an unambiguous description of the geologic structure. This is one of the foremost areas where uncertainty can dictate that another area of concern, which alone would be acceptable, is one too many.

The determination of material properties to +1Rc is straightforward if samples and logs exist to that depth, assuming acceptable quality of the logs and samples. If one or more parameters have gaps in the data, the site geophysicist must determine if the existing data, including projections based on regional knowledge, is sufficient to satisfactorily characterize the site. If the determination is that the data are inadequate, a program of additional logging, sampling, or other studies must be established. If that is not feasible, the site may be restricted to a less than normal yield or to a less flexible choice of WP depth. The practice of reviewing the site from a containment perspective prior to drilling has materially reduced the incidence of such "dog holes".

Very few, if any, holes have a truly complete set of data, and some projection or interpolation is routine in deriving the medium characteristics. The basic question is whether the available information permit a suitable description of the site. Such things as assuming certain properties based on data from a similar lithologic setting are common, and LANL, in fact, does the majority of the site characterization for their "Sand Pile" holes in this fashion. The most common occurrence of a reservation regarding how representative a sample or group of samples is presents itself in establishing the CO_2 content, especially if the averaging interval is relatively small. Since the distribution of CO_2 is not necessarily uniform in the alluvium, there is concern over how well a given sample represents the bulk CO_2 content. This is of particular concern in the 3-4 wt% region, which is not uncommon for alluvium, where containment significance is surfacing, as discussed in Item #11. There sometimes is a question over grain density values, partly because of sampling uncertainties and partly because of the difficulty in making the measurement in the presence of significant zeolite, usually clinoptilolite. Another point to remember is that different averaging techniques are used for different parameters. In particular, if spherical averaging is used, a sample near the end of the interval receives low weight, and an uncertain value is of little consequence. CO_2, since it is assumed to be randomly distributed in alluvium, is arithmetically averaged.

In general, absolute values, with the possible exception of CO_2, are of somewhat less concern than the more qualitative aspects: "How well does the material fit with our understanding of the geologic fabric of the NTS?" and "Are there significant discontinuities?" Does the site fit comfortably within successful experience? If one can answer in the affirmative, then the site is likely to be acceptable. If there are properties that are near the edge or beyond experience, further analysis is necessary, and added restrictions on use of the site may be necessary.

Another request of the CEP is that material properties for the zone 1-2 Rc above the WP be presented. This region is of interest with regard to contrast with the 0-1 Rc region just below. In the same way that there is concern over structure to 2 Rc below the WP, there is even more interest in the zone above because it is nearer the surface. This is the region where the main residual stress field is established. In this region, comparison with other sites is nearly irrelevant; it is the comparison with the region just below that affects event phenomena. This region may sometimes come under scrutiny for CO_2 content if there is concern about that material falling into the cavity region and having its temperature raised enough to release CO_2.

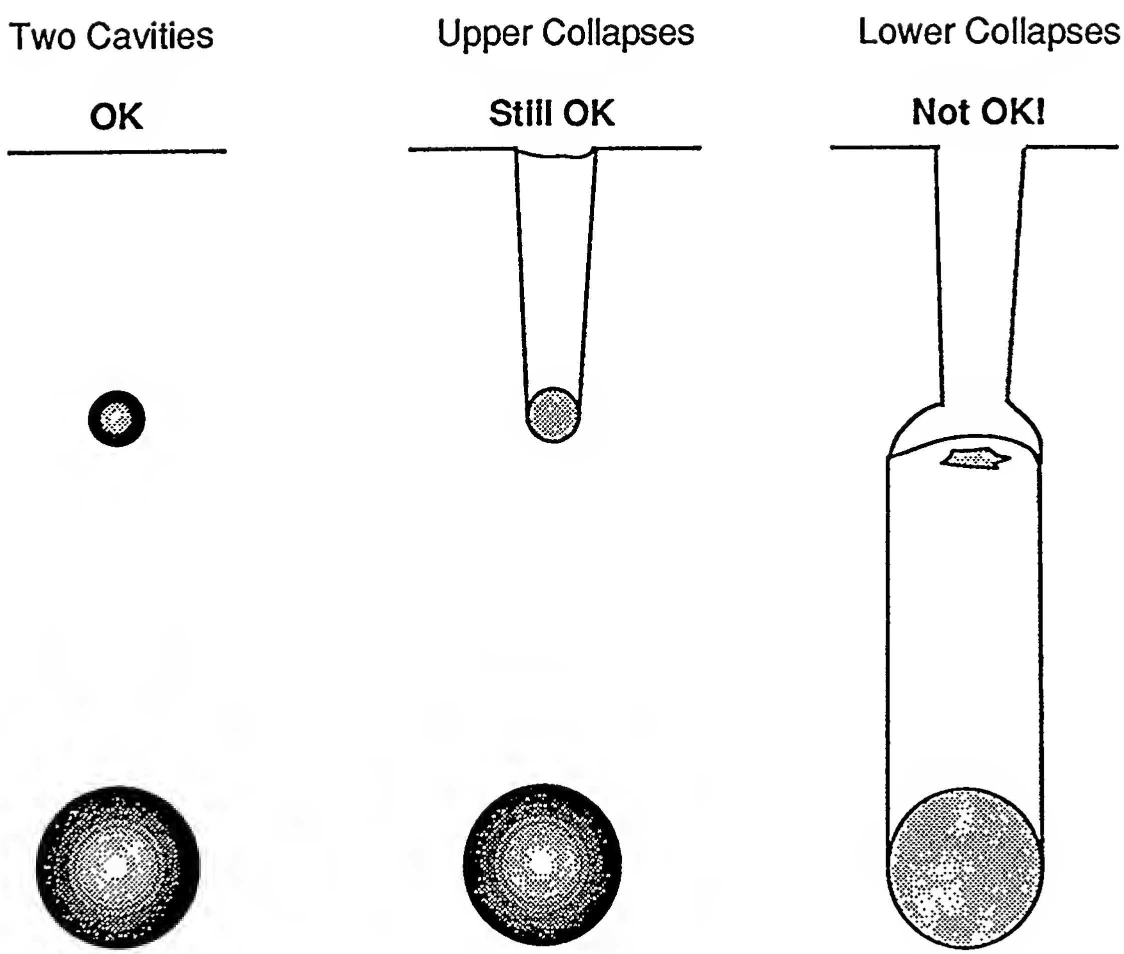
(20) A new drill hole should be 1/2 DOB from an adjacent, collapsed event, and 1 DOB from an adjacent, uncollapsed event.

This rule-of-thumb is rather vague, in that the DOB is not specifically related to either hole in question. The main reason for the lack of definition is that a clearly stated rule would be rather lengthy (as will be demonstrated) and there is no strictly agreed upon prescription. A simple, categorical statement is not available because of the complication of relative depths of the holes. There is a difference whether the existing cavity/chimney is above, below, or beside the proposed test. The difference in treatment of collapsed and uncollapsed tests stems from the difference in how such nearby perturbations could alter the impeded path for gas flow to the surface. The collapsed site is treated as if it is essentially the same as native material. There is some evidence that the gas transport in a fully collapsed chimney is not grossly different from native material in Yucca Flat. This is not generally considered true for Pahute Mesa where fracture flow is much more likely. In addition, that chimney was adequate to prevent flow to the surface from the event that generated it, assuming it was successfully contained, and there is no reason to expect that it would more vulnerable to flow from further away. There has never been an instance where activity that came from one site was detected at the surface at an adjacent site. The few cases where activity has been found in a drill hole are different from the subject at hand; see #25 below.

A site with a subsurface collapse is treated differently since the possibility of a void at the top of the chimney can not, in general, be discounted, and because the site may not be stable. Ground shock from an adjacent test could cause additional collapse, movement of stemming or plugs, etc. Such is not considered likely for a site with a surface collapse.

The separation of 1/2 DOB for a collapsed adjacent site is loosely based on the supposition, qualitatively borne out in tunnel re-entry observations, that beyond about 2.5 Rc there is no obvious alteration of material properties as a result of an explosion; this observation is combined with the earlier stated guideline of keeping the WP at least 2 Rc from a significant material properties mismatch. These, when added, give 4.5 Rc, and, if the cavities involved are of similar size, 4.5 Rc is about 0.5 DOB. This is not a sharp cut-off, and adjacent holes have been closer. The guideline is probably rather conservative, especially if one accounts for differences in WP depth, and can be decreased for specific locations if other concerns permit. Operationally it is often difficult to get much closer since craters that extend to more than 0.3 DOB are not uncommon, and major cracking can extend further. It can be difficult to get a drill rig much closer than the 1/2 DOB guideline when shallow DOBs are concerned, but it can certainly be investigated as a possibility on a case-by-case basis. Wagoner (1991) has pointed out some of the difficulties in siting deep holes; he suggested relaxing the 1/2-DOB separation for a deep event near an expended, shallow, collapsed site. Although he did not specifically propose a revised separation distance, he showed the configuration with a surface separation of 0.12 DOB. The problem with this small a separation is shown in Figure 4; the figure shows zero separation for ease of presentation, but the concern is clear. Collapse of the shallower test followed by what would normally be a subsurface collapse of the deeper test could lead to an unacceptable situation. While not necessarily highly likely, the problem is quite credible: a 60 m radius cavity at 525 m (1722 ft) is at an ROB of 8.75, a reasonably typical value; a 20 m radius cavity at 200 m, also fairly typical, would have a chimney volume, assuming vertical sides, of about 2.3×10^5 m^3 compared with a volume of 9.0×10^5 m^3 for the lower cavity. Even if half the lower volume were lost to bulking, an upper chimney with double the volume calculated for vertical sides could still virtually all fit in an apical void, and the old chimney region would now be a void. One could argue that a lower cavity of the size given would be most unlikely to stop without chimneying to the surface, but a cavity of half that volume could easily produce a subsurface

Figure 4


Two Cavities
OK
Upper Collapses
Still OK
Lower Collapses
Not OK!

collapse and still have a volume comparable to the upper chimney volume. At the very least, the separation must be such as to prevent such an interaction. Craters often have a radius of 2-3 cavity radii, and a crater eccentric from SGZ can extend to 0.4 DOB, which is a major factor in locating trailer parks where they are. The 1/2-DOB guideline can be relaxed on a case-by-case basis, but a significant, across-the-board reduction does not seem prudent.

The separation needed when an uncollapsed site is involved is more conservative due to the uncertainty in the configuration and the possibility that it could change as a result of ground shock. In general an uncollapsed event involves a relatively low yield and is, therefore, likely to be rather shallowly buried. This means that the new site would probably be along side or deeper than the expended site, and the old chimney/void would be in the direction of the surface for the proposed event. Such is the reasoning that has gone into the fairly conservative treatment of such locations. The 1 DOB separation has generally been applied to the newer experiment and ensures that there is no path significantly less than a DOB regardless of the configuration of the old chimney or how it might change during ground shock. This guideline is not strict and is intended as a flag that a site-specific investigation is needed.

(21) The dividing line between surface subsidence and subsurface collapse is at a SDOB of 165 m/kt$^{1/3}$ for Yucca Flat.

The 165 dividing line is obviously not a sharp division, and it applies only to northern Yucca Flat. This is a prime example of a rule-of-thumb, since it is easy to remember, is reasonably useful, and does not require additional data. A somewhat better correlation developed primarily at LANL uses a slightly more complex expression, where the dividing line is at 161 using a density corrected depth, DCD, defined as DOB x ($\partial \div 1.8$), where ∂ is the average overburden density. In effect this says that a more dense rock will behave as if the device were buried more deeply. There is a broad band in which collapse is uncertain. There is an event in Area 2 that did not collapse at a SDOB of 152, and another that did not collapse until +40 months (it was probably knocked down by another event) at a SDOB of 146 m/kt$^{1/3}$. At the other end of the spectrum, an Area 2 event had a normal collapse at a SDOB of 187, and an Area 3 event had a delayed collapse (again probably knocked down) at 5.1 years at a SDOB of 245 m/kt$^{1/3}$. These examples not only illustrate the spread in behavior, but also lend credence to the concern expressed in Item #20 about the possibility of shifting of uncollapsed sites long after one might conclude that a stable configuration exists.

(22) The dividing line between surface subsidence and subsurface collapse on Pahute Mesa is 125 m/kt$^{1/3}$.

The division between surface and subsurface collapse on Pahute Mesa is at a much lower SDOB than for Yucca Flat and the transition zone is somewhat narrower. The generally stronger rocks on Pahute Mesa resulting from the lack of significant thicknesses of alluvium make this intuitively reasonable. "All" shots on Pahute Mesa with a SDOB <118 m/kt$^{1/3}$ have surface subsidence craters, and "all" Mesa shots with a SDOB >132 m/kt$^{1/3}$ have had subsurface collapses. The statistical sample is smaller for Pahute Mesa, which is the reason for qualifying the "all".

(23) A subsurface collapse has a chimney height of 5.5±2.0 Rc.

Subsurface collapses can result from several causes and are not well behaved, as implied by the large transition zone between surface and subsurface collapses and by the error bar stated

in the rule-of-thumb. Chimney collapse is the propagation upward of the void volume of the cavity as a result of the roof material's being unable to support itself. The process can terminate when a strong layer is reached, leaving a void below, or it can terminate if the void volume is taken up by "bulking" of the collapsed material.

Determining the height of a chimney is difficult. The most common method of estimating chimney extent is to measure the electrical length of cables and to equate the location of cable breakage with the top of the collapsed zone. This is an inexact correlation at best, since it assumes, among other things, that the cable is intimately coupled with the formation and will fail at the same level, that the collapse is concentric with the drill hole the cable is in, and that the stretched cable can be measured reasonably accurately. On a few occasions there have been other measurements, such as in a post-shot hole. The relatively poor sampling has given the value in the guideline, 5.5 ± 2.0 Rc for the chimney height, where 2.0 is a 1-sigma value. This is a combination of data from relatively low yields in Yucca Flat and relatively high yields on Pahute Mesa, with little between. The range of estimates is from about 2.5 to about 11 Rc, but the data are often difficult to interpret. Cable breaks in or near a plug are especially ambiguous. The 5.5 value is weighted toward LLNL experience, that is northern Yucca Flat and Pahute Mesa; the mean for southern Yucca Flat, especially Area 3, is likely somewhat higher.

(24.) Plugs in the stemming column should be positioned with #21, 22, and 23 kept in mind.

This is particularly true when a subsurface collapse is likely. The commonest instance is on Pahute Mesa where, at design yield, collapse is unlikely and a strong layer, often in the Rainier Mesa Tuff, is a likely place for collapse to terminate. The plug should be positioned in or just above the strong zone to maximize the stemming kept in the hole. Remember that an unexpected yield can lead to a subsurface collapse of virtually **any height for any event.**

(25) The Grouse Canyon Tuff should be avoided.

The Grouse Canyon Tuff, Tbg, in LLNL use areas tends to be very low-density, high-porosity rock that is relatively weak. The real reason for the concern about Tbg is based on the detection of radioactive material that has traveled for some distance in this particular bed at one location in Area 9. A contaminated zone was found in a new drill hole in 1971, where the activity had apparently traveled rather promptly up a LOS pipe in one hole to the Tbg and then had traveled laterally for at least 172 m. A vertical crack where the activity was concentrated could be detected in downhole photography; it was nearly 10 m in height and was associated with the Tbg. This evidence has led to the tendency to avoid the Tbg where it is present. Because it is also weak and porous (at least it is usually modeled that way), the Tbg can also have a noticeable influence on calculated behavior. The concern is generally if the layer is in the 2-3 Rc range; at this range a rarefaction can be generated. If the Tbg communicates with a fault that goes toward the surface, both concerns are present. When the layer is <2 Rc away, it is simply compacted by ground shock. We have evidence that radioactive material can travel a considerable distance laterally if the properties of the material are suitable, and care must be taken when siting at a location where the Tbg is intersected by the drill hole. At the same time, the potential transport path is sideways, not toward the surface (in the absence of significant faulting), and such behavior is probably acceptable.

(26) Low yield events are more difficult to contain than high yield events.

This is clearly true. The incidence of releases drops rapidly as the yield goes above about 12-15 kt, at comparable SDOB. There have been no releases from high yield events, even when there were no gas blocks or stemming plugs. A treatise could be written on this topic (and probably should be), but, for this purpose, it is sufficient to keep in mind that deviations from the "ideal" site must be given more weight when a low yield test is proposed. There is always the possibility of an unexpected low yield, but the additional depth that goes with a planned large yield is very beneficial. The planned low yield with the concomitant shallow DOB is a hazardous situation, and it warrants conservatism. Another factor with the low yield tests is that there is a higher likelihood of a WP in alluvium with a resulting higher chance of CO_2 contributing to a release.

(27) Unusual craters are a containment hazard.

As a correlation there is some truth here, but cause and effect are not so clear. Unusual collapses tend to be restricted to lower yields; higher yields have normal craters. Releases also are correlated with low yield. Unusual craters are generally thought of as odd-shaped craters, usually with vertical or undercut sides, with depths large for the diameters, and often with the center displaced from SGZ. There is at least one case, AGRINI, where the collapse was so deep for its diameter that it could truly be characterized as having reduced the burial depth. Other strange craters have been dubbed "bottles" and "cookie cutters" to describe there shapes, as opposed to the large dish-shaped craters that are considered "normal". It is likely that the majority of unusual craters are another consequence of the factors that combine to make an event more of a risk, rather than a contributing factor, *per se,* but our understanding of collapse phenomena is limited at best.

(28) A low collapse rate is indicative of possible containment problems.

The collapse process generally is initiated by some series of events, and, once triggered, proceeds fairly rapidly to the surface. The collapse rates generally observed accelerate in a few tenths of a second to a roughly constant velocity of 100-200 m/s. Slow collapses or "staged collapses", that is, those that are a series of separated events, tend to be associated with the transport of radioactive material higher in the hole than is usual or desirable. Such behavior is limited to low yield or high SDOB (roughly >150 m/kt$^{1/3}$) events. Our data on collapse rates is, however, rather limited. We have no way to control collapse rate except as far as we are able to control yield and, hence, SDOB.

(29) Subsurface collapse or a stemming fall can generate a vacuum downhole.

This has been observed on a number of events on pressure transducers in the stemming column. When the stemming material or native material falls away below a given location, there is a transient pressure decrease generated. If the location is near the top of a subsurface collapse, the reduced pressure can persist for a considerable length of time, tens of minutes not being uncommon. There are cases where this condition has apparently hastened the upward transport of radioactive gas from deeper in the chimney, transport that would not have occurred without the void produced by collapse or loss of stemming. A subsequent additional collapse is one of the scenarios that accompanies staged collapses being a containment hazard, as mentioned in #28. There is not much that can be done to prevent this should the SDOB be such that there is not

massive surface collapse, but the possibility should be kept in mind when positioning plugs or considering likely locations for a collapse to terminate, as mentioned in #23.

(30) Surface Effects are not a major containment concern, but they do affect the placing of surface conductors.

Surface effects, the numerous cracks found on the surface as a result of testing, have never been implicated in a release. There is one mandatory condition on the guideline; the cracks must truly be **surface** fractures, that is, they must be shallow and not connected with deeply placed features. The great majority of these cracks are radial or circumferential to a SGZ and die out with distance from the event in question. The most prominent are usually related to a collapse crater. A number of trenches and auger holes have been investigated over the years, and true surface effects are indeed shallow and die out quickly with depth. It is a rare crack that can be traced for more than about 20 m on Yucca Flat. Even the few investigated cracks that apparently pre-date nuclear testing, as evidenced by the presence of caliche, rapidly become indistinguishable with depth, unless they are related to known structure. On Pahute Mesa cracks have been found with low-level radioactivity concentrated along them, as mentioned with respect to "breathing". These cracks are not necessarily structure related in the sense of being fault induced, but they are usually cooling cracks known to penetrate for considerable distances, especially in the Timber Mountain Tuffs. They are not properly classed as surface effects, and they are probably pre-existing. One of the tasks of the Site Geologist is to determine the nature of cracks found at the surface. The reason that surface conductors set for drilling in Yucca Flat are about 35 m deep is that depth was found to be deeper than virtually any surface features, so there was minimal danger of such a crack bypassing a plug in the surface conductor. Shorter surface conductors were used for a short period during the transition from fully cased holes to uncased holes in the 1960s, and in one instance a longer casing had to be set inside the original 23 m long casing of a stockpile hole, U2ao, to make it acceptable to the CEP in 1977. A class of linear surface fractures that trend southwest-northeast appear to be aligned with the regional stress field; some of these have been investigated, and they too appear to be shallow.

There is a class of features, though not necessarily obvious, that seem to be the result of land subsidence and compaction. There are portions of Yucca Flat, most notably in Area 2, that have subsided over 20 feet as a result of ground motion due to testing. Such differential motion of the surface must leave some evidence. Twenty feet corresponds to a compaction of a bit over 1% if one assumes that the compaction is distributed in the porous material above the SWL. In fact, there is probably compaction localized nearer the surface as a result of spall and spall closure induced by testing. Some of the surface cracks that do not seem to be aligned with known geologic structure may be the result of such subsidence. The so-called "ramps" near the north end of the surface expression of the Carpetbag Fault may well have been caused by subsidence as much as by the presence of the fault. There is no consensus regarding the origin of these features or the risk, if any, in siting near them; it is best, in general, to avoid such sites.

(31) In the case of multiples in the same hole, the low yield is better as the deeper WP.

The "string-of-pearls" configuration has been used for a number of years either with two or three very-low yield (sub-kiloton) devices or with a (relatively) large yield on top at a normal DOB with one or two very low-low yield devices below it. The "low below" layout was considered safer from a containment standpoint. One scenario with the lower yield above that was unpalatable to containment is similar to that mentioned in Item #20 and demonstrated in

Figure 4; collapse of a shallow cavity followed by collapse of a deeper cavity could lead to an undesirable configuration. In this case a serious release could result from virtually complete removal of material above the upper WP.

The low-below configuration was definitely preferred by Nuclear Chemistry, to facilitate sampling and to prevent mixing of debris, by Device Systems, who were concerned about the delay time required and the electromagnetic pulse (EMP), and by Prompt Diagnostics, who were concerned about the delay also, in this case to read out data that required several milliseconds. The low yield devices were, in general, buried much deeper than was necessary for the yield involved.

Rather recently we have re-examined the configuration both to assess the possibility of a low yield on top and the possibility of two devices each with significant yield. A series of calculations reported by Rambo and Moss (1991) showed that, at least for some yield and depth combinations, the rarefaction reflected from the surface from a deeper, higher-yield device completely destroyed any residual stress around a shallower, low-yield test. In addition, the situation apparently got worse with decreasing yield at the upper WP, since the smaller yield was less able to "hold its own". As the upper, low-yield device is moved deeper, the magnitude of the rarefaction decreases, but the motion from below distorts and compresses the upper cavity, resulting in considerably higher than normal cavity pressure imbedded in a marginal, at best, stress field. The configuration is still being studied, but the containment position, at least for now, is that, if the low yield is on top or if neither yield is "low", the WPs must be close enough that the cavities merge. This, of course, results in certain mixing of the device debris. Modern modeling capability has, to some degree, justified the concerns that were essentially intuitive some years ago, while pointing the way for some cost saving by still having a low yield shallower than a larger yield.

(32) Some areas have a "reputation".

This is certainly true; the problem is that generalizations have sometimes been allowed to prevail over technical arguments and common sense. The statement is true both for good and bad. The primary example of a good area is the "Sandpile" in Area 3. It is, in fact, a good area, in that it is uniform and has generally good material properties. The classic example of a "bad" area is Area 8, due to BANEBERRY, and, to a lesser extent, the ITS area in Area 9. The BANEBERRY site, U8d, was bad, but the bad features were identified and were avoided in subsequent sites, both in Area 8 and elsewhere. The Area 8 boundary is artificial in that it has nothing to do with the geologic conditions that made the BANEBERRY site treacherous, and assuming a site is worse than another because it is labeled "U8xx" is hardly justified. When the fault zone and the large "pod" of wet, altered tuff are avoided, there are sites that are quite acceptable for a wide variety of yields, as experience has shown. Nevertheless, an event that had been approved by the CEP was denied Execution Authority at DOE Headquarters more than 15 years after BANEBERRY for no apparent reason other than it was in Area 8.

The ITS area had several releases in the late 1960s and 1970 that were mostly the result of lack of modern stemming and gas blocking. The area was, however, rather complex geologically, and probably did not get the careful attention it deserved because of the "production line" atmosphere that went with the ITS concept. It was also the location of the radioactivity found in a new hole related to the Grouse Canyon Tuff mentioned in #25. On the other hand, a

"good" area can lull one into doing a marginal site analysis with the possibility that a surprise is awaiting us.

The lesson is that no matter how convinced one may be of the suitability of a site, there are subjective and, occasionally, even (seemingly) nonsensical arguments that can surface to force additional, apparently unwarranted, conservatism. If one keeps this possibility in mind during the siting and evaluation process and is familiar with the concerns of others in Test Program and at the CEP, the necessary level of conservatism can be built in and additional constraints minimized. Such after-the-fact requirements can be costly. The process can be a difficult balancing act, and knowledge, objectivity, and credibility are necessary.

(33) Some problems are easier to avoid than to solve.

This is not much more than a condensation of the preceding paragraph and others along the way. With the lack of resources to enhance our understanding of the many facets of containment phenomenology and the inherent inability to do control experiments (push it 'til it fails!), containment science has had to rely on experience. Experience has shown us most of the bad things, but it has not given us the understanding to attack many of the gray areas. That being the case, along with the potentially horrendous effects of a containment failure, we are in a position where being prudent and conservative is the optimal course.

CONCLUSIONS

This discussion has treated most topics related to event siting and many topics related to containment evaluation. The main class of topics not covered well are those related to hardware and, to a lesser extent, to stemming. In addition, complex topics that do not lend themselves to development of generalities, such as shock interactions, have usually been given brief mention.

Siting and containment evaluation require optimization of many operational (including financial), technical, and sometimes emotional factors. One must resist the temptation simply to go about the task with a checklist. The checklist would be fine if it were used to make sure that a given item has been considered. The problem is that the site is not governed by our models of it or by our list. It is up to us to understand what can happen and to avoid the unfavorable possibilities; that can only be done with careful attention and consideration of interactions and possible phenomena that can not be readily reduced to a checklist. In a sense, we do not need to predict what will happen, since, at the very least, we do not know what will be the actual yield of the explosive in question. We need to be able to state what will not happen, regardless of the yield actually obtained; what will not happen is a release given any credible set of parameters.

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EXPERIMENTAL DETERMINATION OF CONTAINMENT VERSUS BURIAL DEPTH FOR EXPLOSIONS IN SAND

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ABSTRACT

This paper describes an experimental method for determining the dispersion of the source material following a buried explosion in sand. Using the established technique of subscale centrifuge modeling, both visual and chemical analysis using elements of the explosive as a tracer, results in a dispersion pattern as a function of depth of burst. It is possible to directly measure the percentage of the source material which remains buried and the percentage that settles on the surface close to ground zero. It is not possible, given size constraints of laboratory samples, to measure dispersion of material that becomes airborne and is spread by atmospheric currents. This necessitated choosing depths of burst well past optimum, where crater volume is relatively small, and most of the source material remains below the ground surface.

SYMBOLS

- a spherical radius of explosive charge
- δ mass density of explosive
- DOB depth of charge center below sand surface
- G ratio of model acceleration to terrestrial gravity
- m mass of explosive charge
- V crater volume with original ground surface as reference
- q specific energy of explosive

INTRODUCTION

Subscale testing is an attractive alternative to large scale explosive field testing because of the expense and environmental impact of large field tests. Laboratory experiments can be conducted under highly controlled environmental conditions, and the result is a consistent set of data at a fraction of the cost of a single field test. This assumes that it is possible to scale the experimental model results with a field size prototype. Centrifuge-modeling of explosive and impact cratering has been demonstrated as a viable technique for predicting crater shape and ejecta distribution for large scale events. A detailed discussion of this is beyond the scope of this paper, interested readers are referred to the reference list, especially Schmidt and Housen (1988), Housen, et al. (1993), and Voss and Schmidt (1994). The subject of this paper is an extension of previous work in that previously developed modeling techniques are used, but in addition to measuring crater volume, dispersal of the vaporized source material was also measured. The objective of the experiments was to account for all the vaporized source material, and to determine the depth of burst, as a function of charge mass, where all the source material would remain below the ground surface.

EXPERIMENTAL TECHNIQUE AND DESCRIPTION OF EXPLOSIVES

The experiments were conducted on the Boeing 600G geotechnic centrifuge. This machine has an arm radius of 139.7 cm to base of the swinging platform when fully extended. The swinging platform

allows the use of granular soil targets because the sample surface is horizontal when the centrifuge is stopped, and swings up into a vertical position when the centrifuge is turning. The centrifugal acceleration, G, is the geometric scale factor for the experiments (all experiments were conducted at 488G). For example, field DOB is equal to model DOB multiplied by G, and the field yield is equal to the model yield multiplied by G^3 (assuming the field explosive is the same type as the model explosive). By conducting the model experiment at increased acceleration, the lithostatic stress in the soil of the model matches that in the field prototype, as required by similarity arguments. The similarity for ejecta trajectories in a centrifuge experiment is described in detail by Housen, et al. (1983).

The target material for all the experiments was a uniform quartz sand called Ottawa/Marley #29 mesh. This sand is extremely clean with rounded grains, and has a average grain diameter of 0.43 mm. The target samples were made by pluviating sand into the sample container through a wire mesh sieve. This technique results in a maximum relative target density. Sand dry density for all tests was 1.78 g/cm^3.

Three different explosive sources were used, each with a different tracer element. The first experiments used green DuPont Deta sheet (type C-4). This explosive consists of 63% PETN, 8% Nitro Cellulose (NC), and 29% plastic filler, and leaves a black soot residue after detonation. The soot was used as a visual tracer element. This was discovered during surface burst testing when the white sand sample surface would turn black after detonation. To obtain a quantitative measure of the source dispersion, lead and silver azide explosives (PbN_6 and AgN_6) were used. Upon detonation these explosives release lead or silver metal vapor. The metal oxides condense and are carried by the flow field of the crater formation process. Mapping the resultant metal distribution in the sand then provides a measure of how the source in dispersed below the surface and in the ejecta field. Table 1 is a summary of the specifications of the three explosive charges.

Table 1. Specifications of explosive charges.

Charge designation -	Explosive type -	m Charge mass (g)	q Specific energy (erg/g)	δ Mass density (g/cm^3)	a Charge radius (cm)	Tracer element -
C-4 Deta sheet	63% PETN & 8% NC	0.49	3.85x10^{10}	1.48	0.429	black soot
CILAS-1.7	Lead azide, PbN_6	1.70	1.32x10^{10}	3.10	0.508	71% lead by weight
CISAS-.54	Silver azide, AgN_6	0.54	1.88x10^{10}	4.00	0.318	56% silver by weight

Note: specific energy of TNT = 4.19x10^{10} erg/g, and mass density of TNT = 1.64 g/cm^3.

CRATER VOLUME AS A FUNCTION OF DEPTH OF BURST

Figure 1 shows depth of burst curves for the three different explosive sources scaled to the prototype charge mass and depth of burst. This graph illustrates the effect of specific energy on cratering efficiency. The specific energy of Deta sheet is 92% of the specific energy of TNT, hence the middle curve in Figure 1 can be roughly compared to TNT.

Only one experiment was conducted near optimum depth of burst (the depth of burst corresponding to maximum crater volume for a given explosive). The remainder of the experiments were conducted on the so-called "backside" of the curve, where crater volume approaches zero. (The depth of burst where the crater volume goes to zero is called "critical depth".) DOB's close to critical were selected to minimize the amount of source material that would be ejected from the sand sample. In this way the experimental constraints, ie., sample size, would not greatly influence the results. This was also the

case of greatest interest since one objective was to determine the DOB required for complete containment.

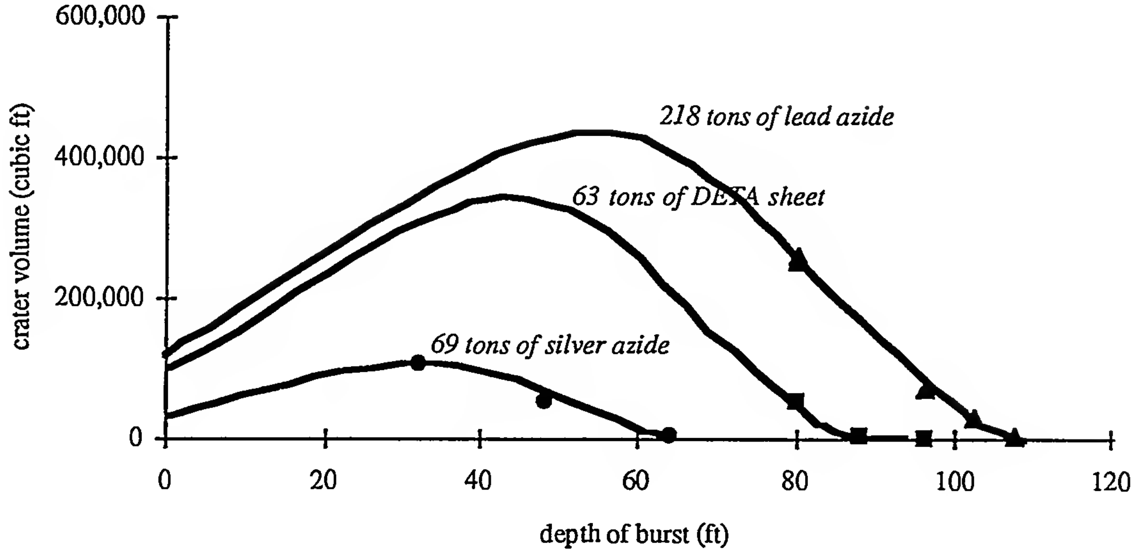

Figure 1. Prototype crater volume as a function of depth of burst.

DISPERSION OF SOOT FROM DETA SHEET EXPLOSIVES

Deta sheet explosive produces a black soot residue upon detonation. The soot particles are dispersed in the ejecta field at shallow depths of burst, coat the surface of the crater at depths of burst beyond optimum, and remain below the surface at critical depth. Assuming that the soot is a measure of source dispersal, three initial experiments were conducted with the main objective of determining suitable depths of burst for the more complicated chemical tracer experiments. The surface of each sample was examined for any soot residue. This was not difficult because the sand is white before firing the explosive. Additionally, the subsurface distribution was obtained by pushing a 1/16 in. thick piece of glass down through the centerline of the crater and excavating the sand on one side. Figures 2 through 4 show photographs of the results for the three depths of burst. The column of photographs on the left side of the page are the surface views. Note that the diameter of the model sample surface shown is 11-1/4 inches. The the column of photographs on the right side are the subsurface views seen in cross section. The white area shown in the subsurface view is pulverized sand around the charge. The streaks below this are smears of soot made on the glass when it was pushed into the sand.

Figures 2 through 4 illustrate the sensitivity of source material dispersion to depth of burst. The 80 foot depth of burst, shown in Figure 2, results in a majority of the soot remaining below the surface or on the surface inside the crater, but there is some soot on the surface outside the crater lip crest. It was not possible to quantify the amount of soot that settled outside the crest of the lip; however, the residue did not extend beyond a scaled range of 152 feet, which is less than the 170 foot outer radius of the crater lip. So in this instance, it appears that the soot did not become airborne and was limited to the range of the ejecta. An increase in depth of burst to 88 feet, shown in Figure 3, results in all the soot remaining either below the surface or on the surface inside the crater lip crest. In this case the crater volume is extremely close to zero (see Figure 1). At a depth of burst of 96 feet the crater volume is zero, and as shown in Figure 4, all the soot remains below the surface. Based on these preliminary experiments, the chemical tracer experiments were conducted at DOB's close to critical depth.

RESULTS OF CHEMICAL TRACER ELEMENT EXPERIMENTS

Lead and silver azide explosives were used to obtain a more quantitative assessment of source material

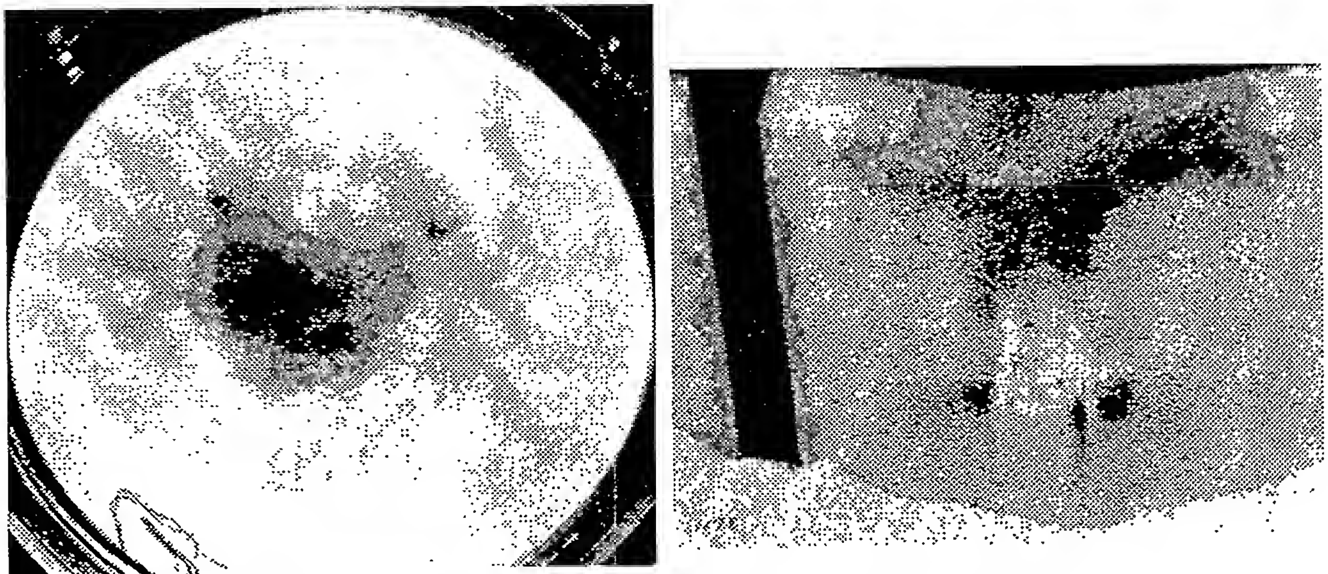

Figure 2. Dispersion of soot for a 63 ton DETA sheet explosive buried 80 feet.

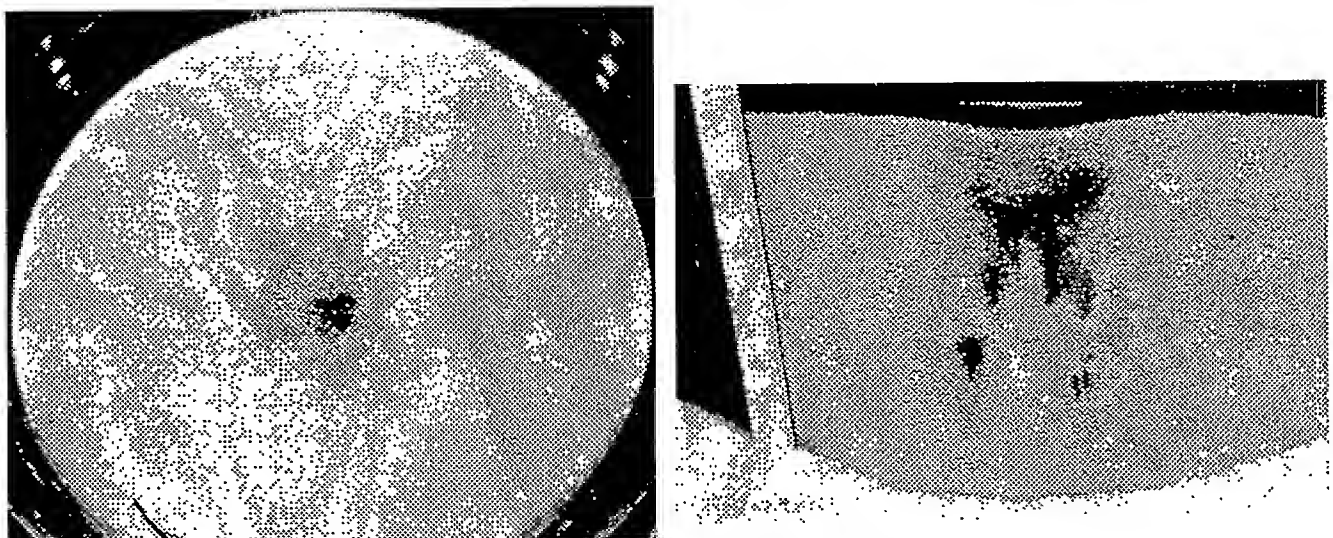

Figure 3. Dispersion of soot for a 63 ton DETA sheet explosive buried 88 feet.

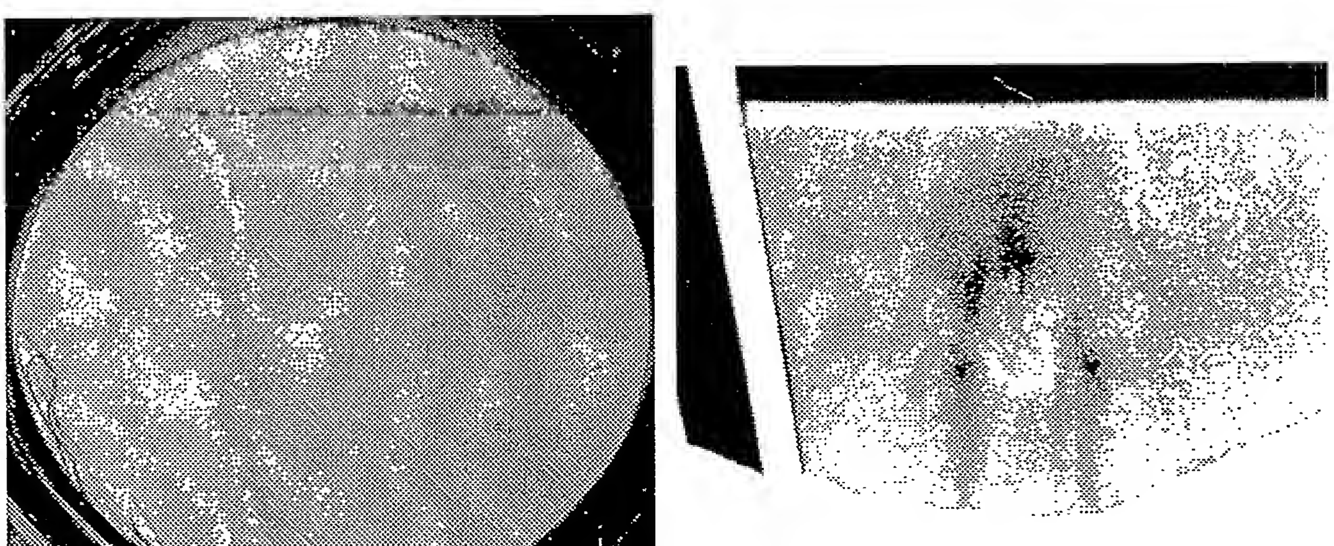

Figure 4. Dispersion of soot for a 63 ton DETA sheet explosive buried 96 feet.

dispersion. By analyzing the sand for metal content after detonation, the dispersion of the source material was plotted as a function of position on the sample. Figure 5 shows the sample locations. The container was sealed with a lid during the experiment to prevent any loss of source material.

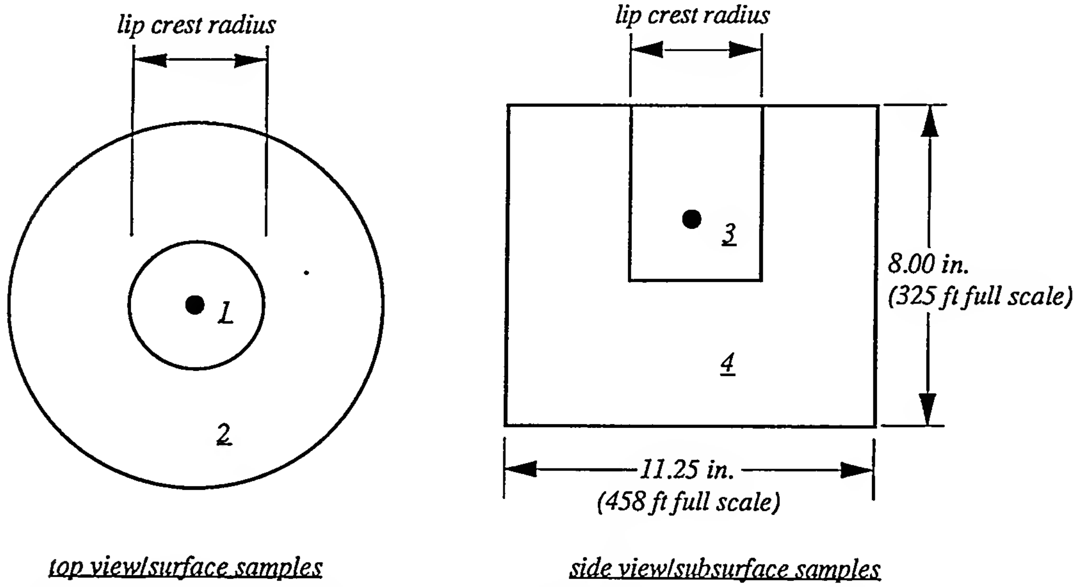

Figure 5. Sampling zones for tracer chemical analysis.

A standard chemical analysis technique used for toxic waste monitoring was used to measure metal content of the sand. Concentrations of metal as low as 30 parts per billion can be quantified by using an inductively coupled argon plasma spectrometric analysis (ICAP). An acid bath procedure is used to dissolve the metal from the sand samples, after which the spectrometer is used to determine the total metal concentration in each sample.

The unused sand was tested for metal content before any explosive experiments were conducted. This pretesting showed less than 0.1 parts per million (ppm) of lead and less than 0.02 ppm silver in the sand. This is well below the expected experimental concentrations, which are on the order of 100 ppm. As a further calibration of the analysis technique, lead or silver powder was added to a known quantity of sand. Eighty-six percent of the metal powder was recovered by the acid bath and ICAP analysis. A second calibration was conducted with contained lead and silver azide explosives in a known quantity of sand. Seventy-five percent of the metal was recovered in this case. Both of these calibration tests used a 1 kg sample of sand, and the entire sample was rinsed with acid to produce the solution used in the ICAP analysis. So the loss of 14% of the metal in the powder test was probably due to losses in the rinsing process of the relatively large sand sample. The additional 11% loss for the explosive calibration was probably due to some insoluble metal compounds formed by the explosion. Smaller sand samples, on the order of 20 g, can be completely digested with acid, and presumably this would recover more of the metal.

The results of the chemical analysis are plotted in Figures 6 and 7. These plots show percent of metal recovered as a function of the zones shown in Figure 5. Each bar on the chart corresponds to a different depth of burst.

Not all of the source material was recovered. The quantity of missing metal is greater for the silver azide experiments, as shown in Table 2. Zone 3, the cylindrical volume of sand around the explosive, contained much higher concentrations of source metal than the other zones. This zone contained from 3 to 6 kg of sand, which meant that the acid rinse technique had to be used. On the other hand, the surface zone samples, zones 1 and 2, were on the order of 20 to 50 g, and the acid digestion method was used. So, based on the presently available data, the zone 1 and 2 results must be considered more reliable than

the zone 3 results, and the zone 3 results are considered a lower bound. The preliminary conclusion is that the "missing" source material is located in zone 3, but that it was not detected due to losses during the acid rinsing procedure.

Table 2. Summary of chemical tracer analysis.

Shot number -	Explosive type -	Model DOB (cm)	Full scale DOB (ft)	Surface (zone 1 + 2) tracer recovered (%)	Subsurface (zone 3 + 4) tracer recovered (%)	Sum of tracer recovered (%)
1204	PbN_6	5.0	80	3.63	69.88	73.51
1205	PbN_6	5.0	80	3.83	70.58	74.41
1206	PbN_6	6.0	96	3.48	62.01	65.49
1207	PbN_6	6.4	102	1.83	46.49	48.31
1208	PbN_6	6.7	107	0.53	58.21	58.73
1211	AgN_6	2.0	32	9.98	33.04	43.02
1210	AgN_6	3.0	48	5.46	19.88	25.35
1209	AgN_6	4.0	64	0.26	37.68	37.94

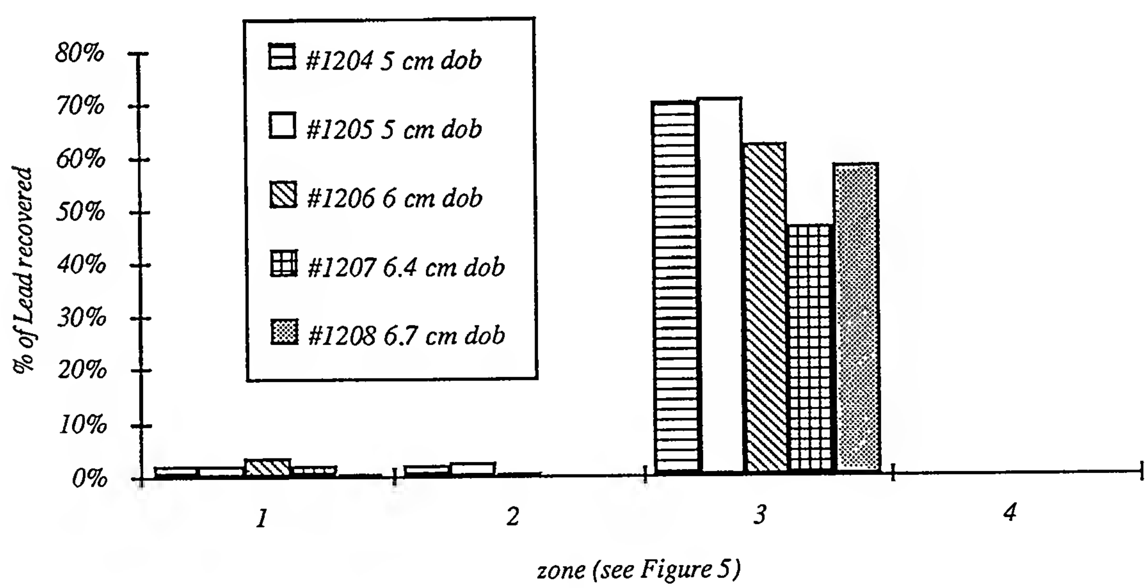

Figure 6. Results of chemical analysis for lead azide explosive.

The lead azide experiment at 5 cm (80 ft) DOB was repeated once, and the two results agreed as shown by Figure 6 and Table 2 (shots 1204 and 1205). The overall trend is, as expected, the amount of source material recovered on the surface decreases as the depth of burst approaches critical. The results from Figures 6 and 7 are replotted in Figure 8 as percent of the source material recovered on the surface, sum of material in zones 1 and 2, versus prototype scale depth of burst. The crucial point of this figure is that near critical depth less than 1% of the source material was recovered on the sample surface. This value increased to 10% at optimum depth of burst for the silver azide explosive. This plot contains the data with the highest confidence level because the samples were small enough to use the acid digestion technique.

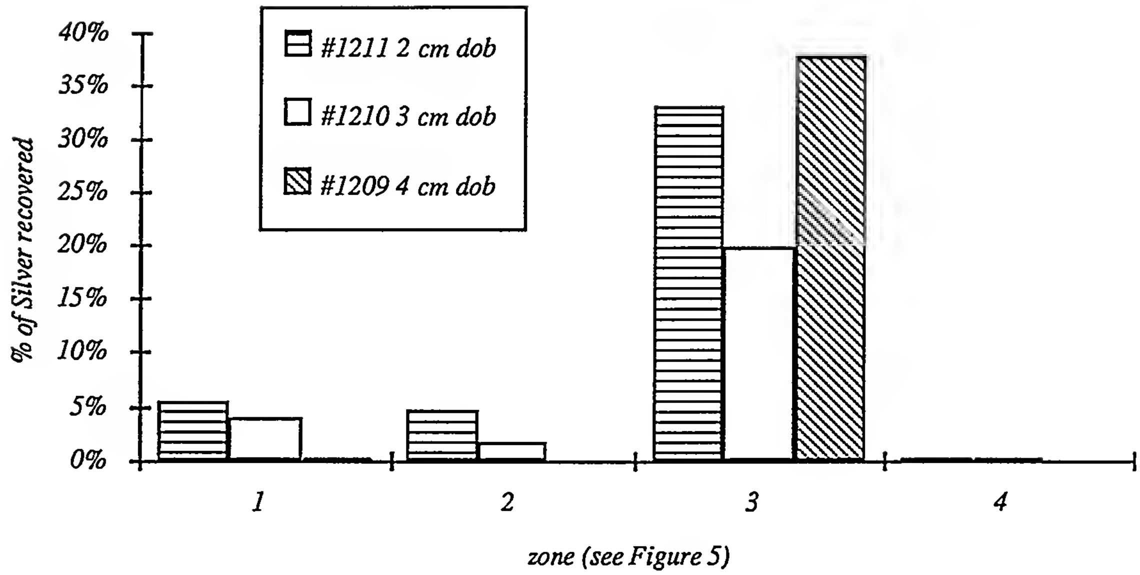

Figure 7. Results of chemical analysis for silver azide explosive.

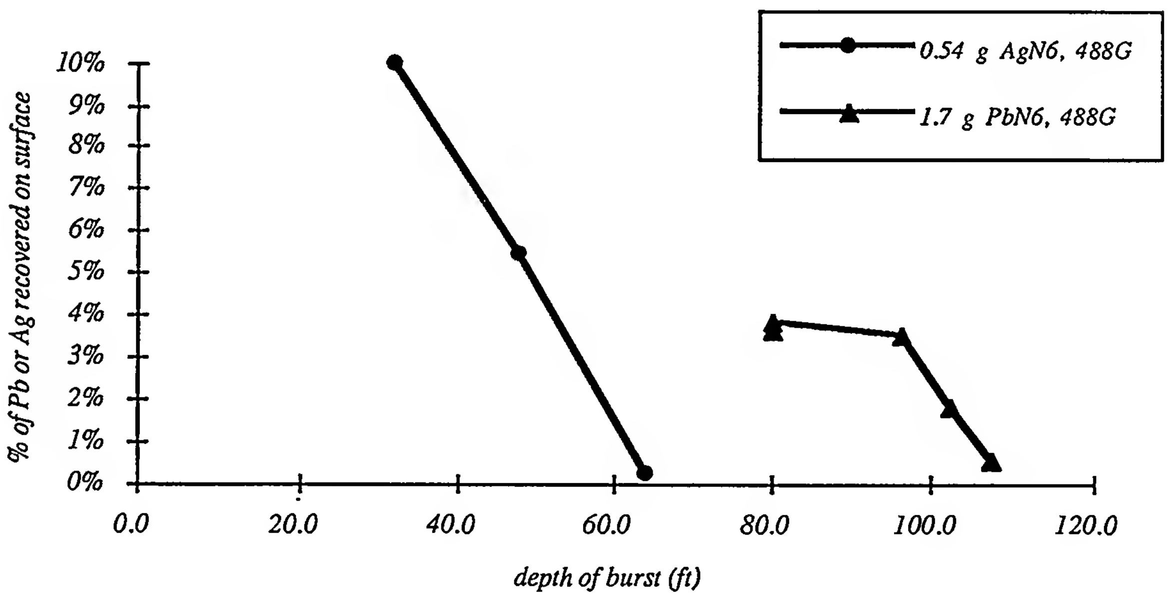

Figure 8. Explosive dispersion on the surface as a function of depth of burst (prototype scale).

CONCLUSIONS

This paper describes a series of experiments conducted to determine the dispersion of explosive source material for a buried explosion in sand. The objective is to develop a method for predicting the depth of burst required for full containment of the explosive source material. Laboratory techniques were developed to use the metals in lead and silver azide explosives as tracers, and by measuring the metal concentrations in the sand after the explosions, source dispersion was determined. The experimental techniques used are presently evolving, and if future work is done it will focus on accounting for all of the tracer metal, thereby increasing confidence in the results. A conclusion, albeit preliminary, is that near critical depth of burst less than 1% of the tracer elements are deposited on the sample surface. Therefore, full containment requires depths of burst greater than critical depth.

ACKNOWLEDGEMENTS

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Scaling of Deeply Buried Explosions

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Abstract

The containment of underground explosions involves many of the same problems encountered in the related problem of explosive cratering. In particular, for both cases, field tests are often conducted with yields and burial depths vastly different from those of actual interest. Therefore, the question of scaling must be addressed before field results can be usefully applied.

The scaling of explosions has been a subject of interest for many years, as evidenced by the work of Lampson (1946), Sedov (1959), Chabai (1959), Vaile (1961), Nordyke (1962), and others. These early efforts predicted the existence of two distinct regimes in which the outcome of an explosion is determined by either material strength or by the lithostatic overburden. Dimensional analysis was used to argue that crater size, when normalized by a power of the yield, $W^{1/n}$, is a function of the burial depth normalized by $W^{1/n}$, and that $n = 3$ and $n = 4$ in the strength- and the gravity-dominated regimes, respectively.

In contrast to the predictions, empirical analyses of field data by Chabai (1959) and Nordyke (1962) gave a value of $n = 3.4$, intermediate between the cube-root and quarter-root values. Chabai suggested this might be the result of the combined effects of gravity and atmospheric pressure (which, like strength, gives a value of $n = 3$).

In the two decades following the initial work in the early sixties, little progress was made in understanding how the yield exponent is affected by pressure, strength, and gravity, and what its asymptotic values should be in the limiting cases in which one of these mechanisms dominates over the others. Two exceptions are the work of Johnson et al. (1969) and Herr (1971), which both showed, through experiments, that pressure is an important factor in cratering of cohesionless soils, but did not specifically address the scaling problem.

We have conducted explosion experiments on the Boeing centrifuge in order to determine the scaling exponent, n, for large yields, and how this exponent is affected by ambient pressure, gravity, and material strength. The centrifuge operates at centripetal accelerations of up to 500 G, thereby providing an effective variation in the explosive yield of more than 10^8 (i.e., 500^3). Our initial tests were conducted in dry sand inside a fixture that mounts on the centrifuge and that can accommodate ambient pressures of roughly 10^{-3} to 30 atm. Using this fixture, independent variations can be achieved for the ambient atmospheric pressure, charge yield, and burial depth.

A series of experiments were performed to determine the scaling exponent for large yields, i.e., in a regime in which strength and atmospheric pressure effects are absent. To accomplish this, the tests were conducted in cohesionless dry sand under near-vacuum conditions. Both the charge burial depth and the centripetal acceleration (i.e., scaled yield) were varied. These tests provided the first results in a purely gravity dominated regime with a significant variation (a factor of 600) in the explosive yield. Plots of the scaled crater volume, $V/W^{3/n}$ *versus* the scaled burial depth, $d/W^{1/n}$ showed that the exponent n is 3.7, which is significantly larger than the value of 3.4 deduced from field tests, yet still below the quarter-root value of 4.[1]

Subsequent experiments have shown that, when ambient pressure effects are important, the scaling exponent is smaller than its gravity-regime value. In particular, for 1-atm conditions, the exponent should be in the range of 3.4 to 3.5 for yields in the range of typical field tests, i.e., tons to kilotons. These results suggest that $n = 3.4$ is not appropriate for large yields.

[1]The derivation of quarter-root scaling is flawed by the incorrect assumption that the explosion outcome is determined by the explosive energy, but is independent of its specific energy. When specific energy is important, the gravity-regime scaling exponent cannot be 4 (Schmidt et al. 1986, Holsapple and Schmidt, 1987).

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Tunnel and LOS Topics

LOW-YIELD EVENT DESIGN AND PERFORMANCE REVIEW

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ABSTRACT

The low-yield horizontal-line-of-sight (HLOS) effects tests conducted by DNA use a HLOS drift containment system design that is different than that used on previous standard-yield tests. Mechanical pipe closures for a low-yield test include the fast-acting-closure (FAC), gas-seal-auxiliary-closure (GSAC) and tunnel-and-pipe-seal (TAPS) with the closure nearest the working point (*i.e.* FAC) designed to also serve as a stemming bulkhead. Alternate lengths of rock-matching and superlean grouts comprise the HLOS drift stemming column. In contrast, previous standard-yield tests used a modified-auxiliary-closure (MAC), GSAC and TAPS. In that design, the closure nearest the working point (*i.e.* MAC) was not designed, or positioned, to serve as a stemming bulkhead. In addition, the standard-yield stemming column consisted of a single length of rock-matching grout beginning in the region of the working point followed by a length of superlean grout extending to the MAC.

Containment of the low-yield events has been outstanding. There has been no accidental release of radioactivity to the atmosphere and no leakage to the tunnel complex, Vessel 3. Operationally insignificant quantities of activity have been detected in the tunnel complex, Vessel 2, on a few events and some minor quantities of cavity gas have, on occasion, seeped into the LOS pipe.

All low-yield tests have qualitatively similar containment system designs. However, overall test details are unique with design differences associated with device yield, test objectives, site geology, etc. In total, twenty three design features have been identified. These design features are examined for differences that may correlate with observed seepage to the HLOS pipe and/or Vessel 2 of the tunnel complex.

1 INTRODUCTION

The Defense Nuclear Agency (DNA) uses a three-vessel containment system concept, within the tunnel-complex, when conducting underground horizontal-line-of-sight tests. As shown on Figure 1, Vessel 1 includes the cavity and tunnel volume contained within the stemming plug located in the HLOS drift. Vessel 2 is the large volume of the tunnel complex contained within the Drift Protection Plugs (DPPs). Vessel 3 is the region of the tunnel complex contained within the Gas Seal Plugs (GSPs). Vessels 2 and 3 are redundant containment features each designed to be capable of containing the cavity gas should Vessel 1 catastrophically fail. Containment in the vertical direction is provided by the overburden.

The low-yield HLOS effects tests conducted by DNA during the last numbers of years use a HLOS drift containment system (*i.e.* Vessel 1) design that is different from that used on previous standard-yield tests. Design modifications were necessary since the length of the ground-shock-induced stemming plug is idealized as proportional to the cubed root of the yield whereas the driving conditions for gas seepage, namely the cavity gas pressure and temperature, remain nearly independent of yield. The design goal for containment of the low-yield events was to, therefore, develop a containment system wherein a shorter but competent stemming plug would be formed with great confidence.

A typical HLOS Vessel 1 containment system is illustrated on Figures 2 and 3. Major design changes for the low-yield events, as compared to the previous standard yield events, include 1) the use of a fast-acting-closure (FAC) designed to both block the HLOS pipe flow and to serve as a stemming bulkhead 2) the placement of alternate lengths of rock-matching grout (RMG) and superlean grout (SLG) in the stemming region in order to prevent excessive extrusion of the weak SLG, and 3) a reduction in the HLOS drift cross-sectional area in the region of the WP so that the solid angle subtended by this drift would be no larger than that of the standard yield tests. Additional mechanical closures include the gas-seal-auxiliary-closure (GSAC) and tunnel-and-pipe-seal (TAPS) used on the standard-yield tests.

In contrast, previous standard-yield tests used a modified-auxiliary-closure (MAC) plus a GSAC and TAPS. In that configuration, the closure nearest the working point (*i.e.* MAC) was not designed, or positioned, to serve as a stemming bulkhead. In addition, the standard-yield test stemming column typically consisted of a single length of rock-matching grout beginning in the region of the working point followed by a length of superlean grout extending to the MAC.

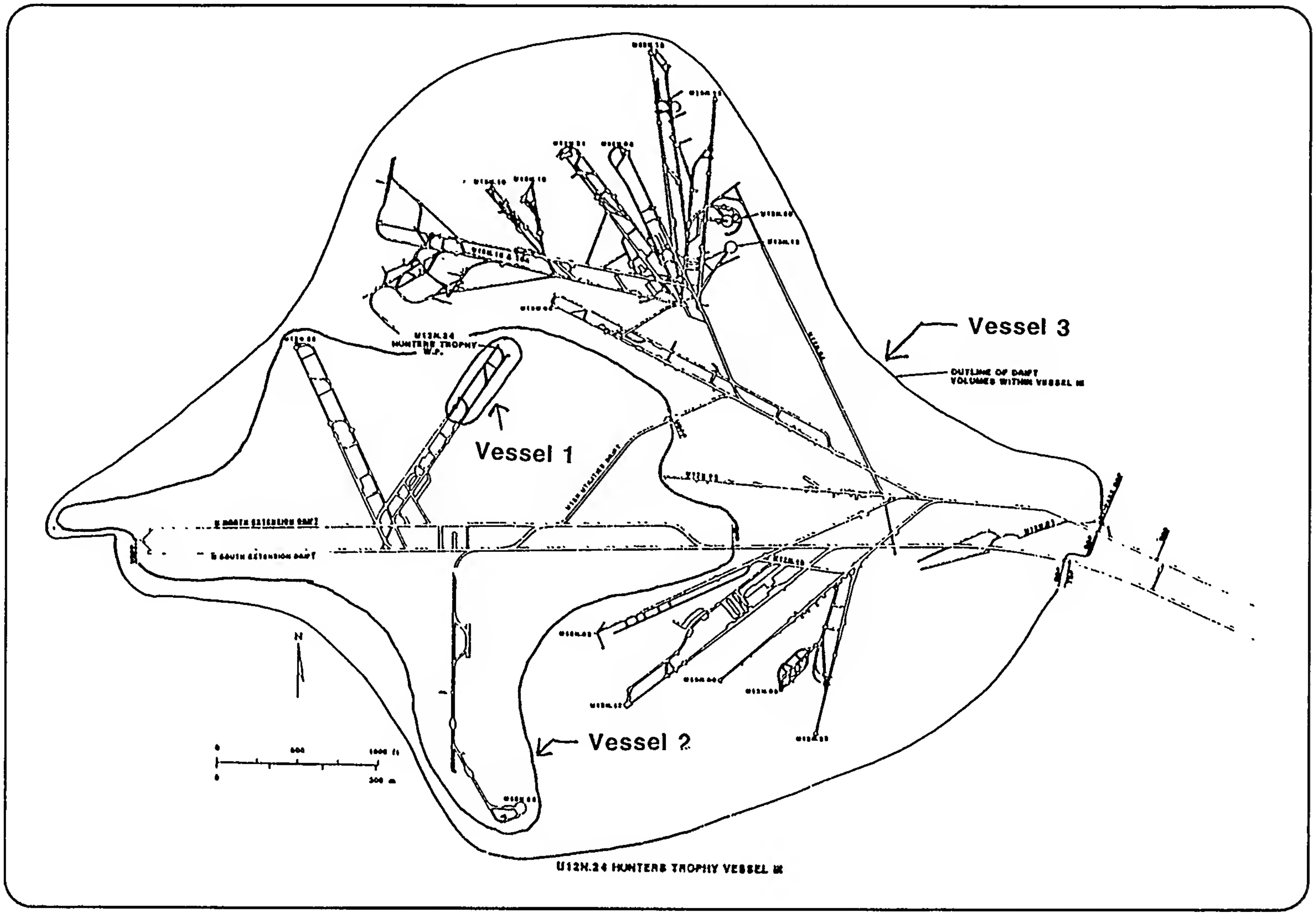

Figure 1. Vessel 1, 2 and 3 layouts for the U12n.24 HUNTERS TROPHY event.

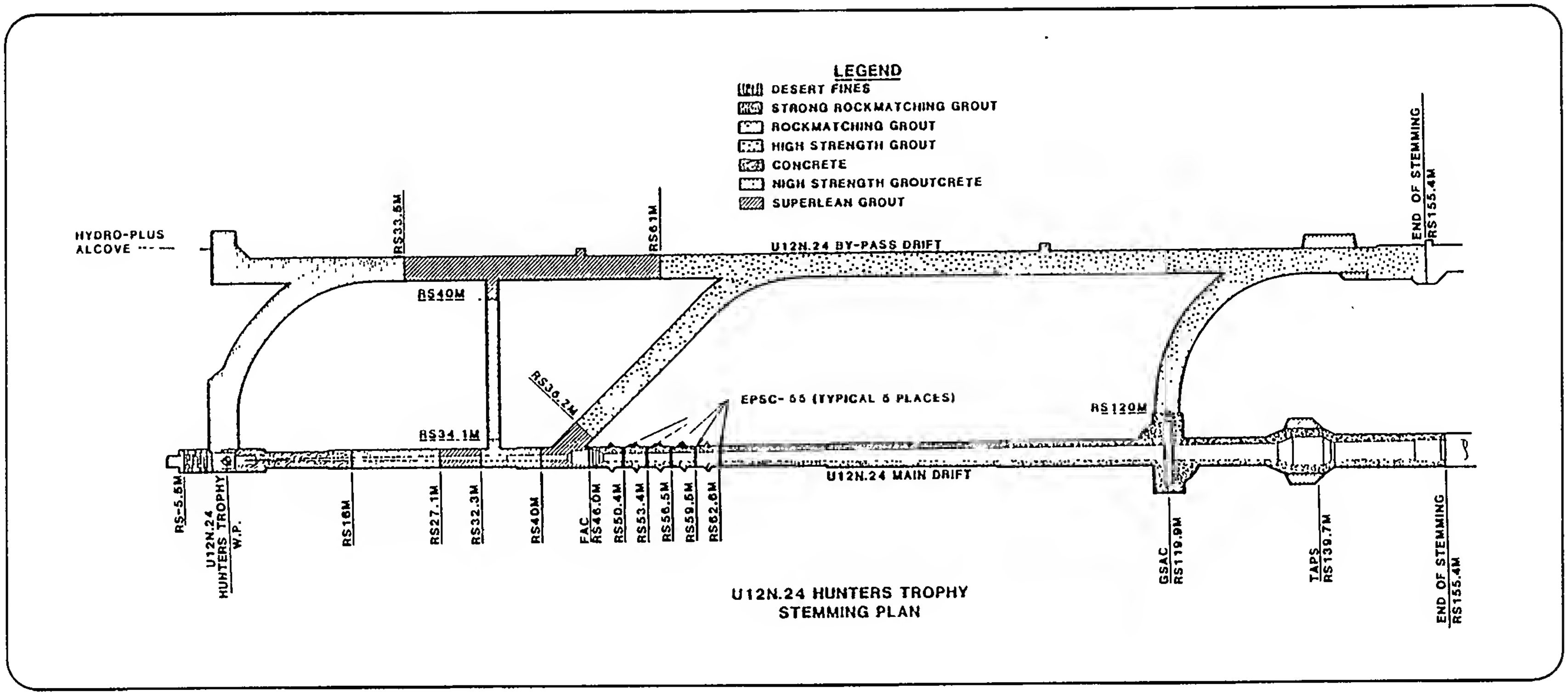

Figure 2. Stemming design for the HUNTERS TROPHY event.

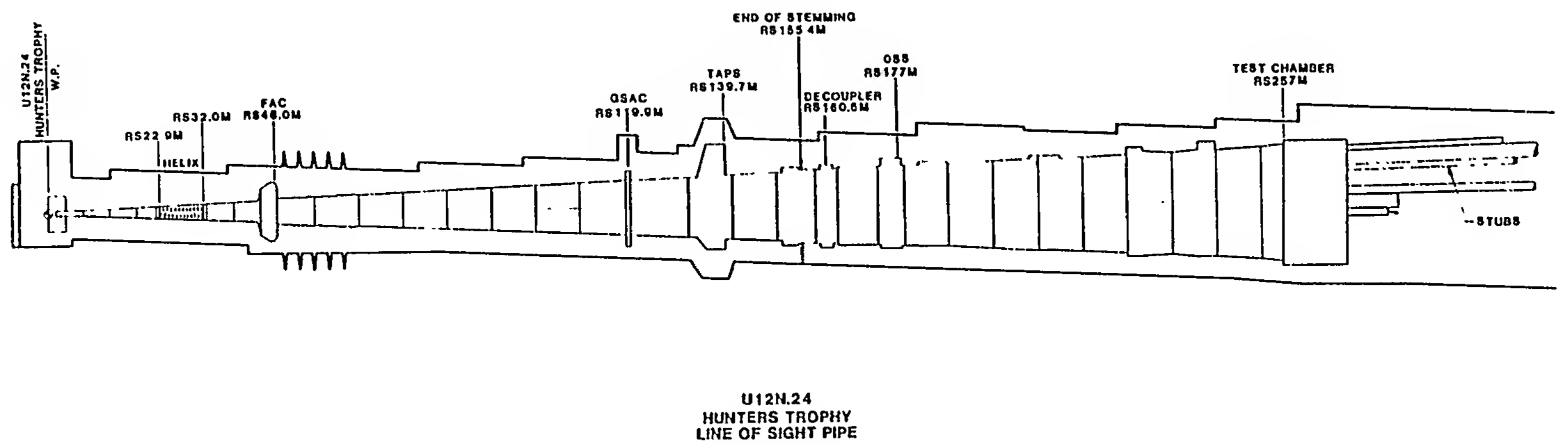

Figure 3. HUNTERS TROPHY LOS pipe, test chamber and mechanical closures.

Containment of the low-yield events has been outstanding. There has been no accidental release of radioactivity to the atmosphere and no leakage to the tunnel complex, Vessel 3. Operationally insignificant quantities of activity have been detected in the tunnel complex, Vessel 2, on a few events and some minor quantities of cavity gas have, on occasion, seeped into the LOS pipe.

The primary intent of this study is to compare the test design and test performance, information for possible correlation with the observed small seepages. A secondary intent is to present this test information in a format that will enable readers to form their conclusions concerning design/performance relationships.

Although all low-yield tests have qualitatively similar containment system designs, overall test details are unique with design differences associated with device yield, test objectives, site geology, etc. These design features are compared in Section 2. Containment related ground motion and containment structure performances are summarized in Section 3. Observed LOS pipe and tunnel complex Postshot Environments are described in Section 4. Conclusions are presented in Section 5.

This review will focus on the Vessel 1 containment system whose satisfactory performance is required for experiment recovery and instrumentation protection purposes. There has been no threat to the Vessel 2 or 3 containment structures on any low yield event and the containment features of these Vessels will not be discussed in this paper.

2 CONTAINMENT SYSTEM DESIGN COMPARISON

Eight low-yield tests have been conducted. MIDNIGHT ZEPHYR (MZ) and DIAMOND BEECH (DB) were containment system proof tests. MIDDLE NOTE (MN), MISSION CYBER (MC), DISKO ELM (DE), MINERAL QUARRY (MQ), DISTANT ZENITH (DZ), and HUNTERS TROPHY (HT) were effects tests. For the purposes of this study, the MN containment design is taken as the standard for comparison.

The Vessel 1 containment system designs for these eight low-yield HLOS tests are compared on Figure 4. As shown, twenty-three features have been identified that may affect containment performance. Detailed explanations of the design differences are provided in Table 1 where the item numbers correspond to the numbers appearing at the bottom of the columns found in Figure 4.

On Figure 4, an **open** box indicates that design feature was identical to that of MIDDLE NOTE. A **partially shaded** box indicates the design feature was somewhat different than that of MIDDLE NOTE while a **fully shaded** box indicates the feature was significantly different than MIDDLE NOTE. The **absence** of a box indicates the feature did not exist on that test. Please note that the partial or full shading of a box indicates the design was different, not better or worse, than that of MIDDLE NOTE.

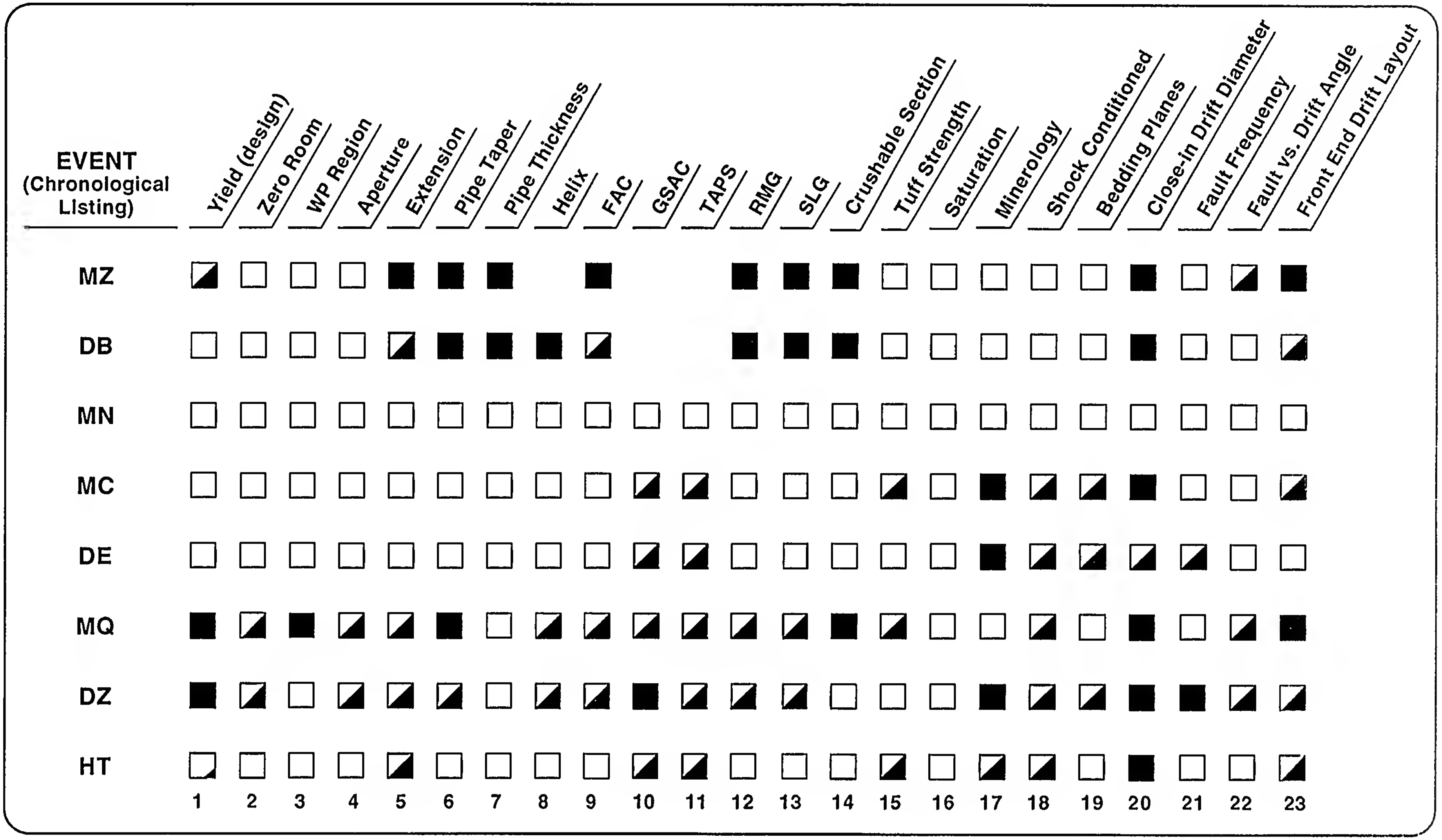

Figure 4. Comparison of the low-yield LOS event containment system designs.

Table 1. Containment System Design Comparison for the Low Yield LOS Events (Explanation of notes on Figure 4).

Note	Explanation
1.	On MIDNIGHT ZEPHYR the design yield was somewhat less than that of MIDDLE NOTE. The MINERAL QUARRY and DISTANT ZENITH design yields were larger than that of MIDDLE NOTE. The HUNTERS TROPHY design yield was slightly larger than MIDDLE NOTE.
2.	MINERAL QUARRY and DISTANT ZENITH use LANL devices while the other events used LLNL devices. The designed zero room energy density was similar for all events.
3.	The Randsburg experiment was located near MINERAL QUARRY. Open volumes in the region of the working point were small for all events.
4.	The aperture diameters on MINERAL QUARRY and DISTANT ZENITH were 13-percent larger than on the other events.
5.	MIDDLE NOTE, MISSION CYBER, and DISKO ELM had tungsten extensions with external iron liners to provide the vacuum seal. MINERAL QUARRY, DISTANT ZENITH and HUNTERS TROPHY had Tuballoy extensions with internal iron liners. DIAMOND BEECH had a tungsten extension with an internal iron liner. MIDNIGHT ZEPHYR had an iron extension with an internal iron liner and this lower density extension is calculated to produce an increased pipe flow. The MIDNIGHT ZEPHYR and DIAMOND BEECH extension lengths were 71-percent of that of MIDDLE NOTE. MISSION CYBER, DISKO ELM, and HUNTERS TROPHY. The lengths of the MINERAL QUARRY and DISTANT ZENITH extensions were yield scaled to that of MIDDLE NOTE.
6.	MIDNIGHT ZEPHYR and DIAMOND BEECH had a 1.90 cm/m taper. MIDDLE NOTE, MISSION CYBER, DISKO ELM and HUNTERS TROPHY had a 1.42 cm/m taper. MINERAL QUARRY had a 1.01 cm/m taper while DISTANT ZENITH had a 1.23 cm/m taper.
7.	MIDNIGHT ZEPHYR and DIAMOND BEECH had a 6.35-mm-thick pipe between the end of the extension and the FAC. The other low yield events had a 4.8-mm-thick pipe. The thinner pipe and helix result in an approximate 40-percent reduction in the pipe/helix iron mass.
8.	The 4.8-mm-thick MIDDLE NOTE helix was located between RS 23 m and RS 32 m. It had a thickness equal to that of the pipe wall, a width equal to the approximate pipe diameter, and a pitch approximately three times its width. The range of the MINERAL QUARRY and DISTANT ZENITH helices were yield scaled to that of MIDDLE NOTE. The DIAMOND BEECH helix ran from the end of the extension to the FAC. Its thickness, width, and pitch were steadily increased from their 6.55 mm, 19.7 mm and 457 mm values at the end of the extension to their maximum values of 15.9 mm, 533 mm, and 787 mm values at the FAC. The smaller helix was introduced, beginning with MIDDLE NOTE, in order to lower the LOS pipe iron content and thereby limit potential H_2 and CO production, and to insure that the pipe was not so massive as to preclude closure.
9.	The MIDNIGHT ZEPHYR FAC was closed prior to installation. The MIDNIGHT ZEPHYR and DIAMOND BEECH FACs were located at RS 33.5 m. The MIDDLE NOTE, MISSION CYBER, DISKO ELM and HUNTERS TROPHY FACs were located at RS 46.2 m so that they would be outside the two cavity radii position within which unpredictable non-radial motion of materials is characteristic of the DNA experience. The FAC positions on MINERAL QUARRY and DISTANT ZENITH were yield scaled to that of MIDDLE NOTE. Because of the higher yield, the impulse on these FACs was larger.

Table 1. Continued

Note	Explanation
10.	The scaled range to the GSACs on the low yield tests is at least 50-percent larger than on the standard yield tests. The range from the working point to the GSAC on MISSION CYBER, DISKO ELM, and HUNTERS TROPHY is 23 m larger than on MIDDLE NOTE. The range to the GSAC on DISTANT ZENITH scales to that of MISSION CYBER while the range on MINERAL QUARRY scales approximately midway between that of MIDDLE NOTE and MISSION CYBER. A proof test of a Stemming Anchor Closure (StAC) was performed on DISTANT ZENITH. The StAC was located between the FAC and GSAC.
11.	The scaled range to the TAPS on the low yield tests is at least 40-percent larger than on the standard yield tests. The range from the working point to the TAPS on MISSION CYBER, DISKO ELM, DISTANT ZENITH and HUNTERS TROPHY is 25 m larger than on MIDDLE NOTE. The range to the TAPS on MINERAL QUARRY scales approximately midway between that of MIDDLE NOTE and MISSION CYBER.
12.	The MIDNIGHT ZEPHYR LOS drift was stemmed with MJ-2A, a strong RMG (24 MPa unconfined strength) out to 28 m, DSRM-2 RMG (6.8 MPa unconfined strength) from RS 28 m to RS 32 m, and HSSL-1A SLG (1.4 MPa unconfined strength) from RS 32 m to the FAC located at RS 35.4 m. The DIAMOND BEECH LOS drift was stemmed with DBRM-2 RMG (6.8 MPa unconfined strength) out to 22 m with ME8-05 RMG (i.e. 21 MPa unconfined strength) from RS 22 m to RS 27 m, DSRM-2 from RS 27 m to RS 31 m and HSSL-1A from RS 31 m to RS 35.7 m. The MIDDLE NOTE LOS drift had ME8-05 out to RS 16 m, DSRM-2 from RS 16 m to RS 27 m, HSSL-50/50 from RS 27 m to RS 34 m, DSRM-2 from RS 34 m to RS 40 m, and HSSL-50/50 from RS 40 m to RS 46.3 m. The stemming designs on the subsequent events were yield scaled to that of MIDDLE NOTE. The stemming design on MIDDLE NOTE was changed from that of the proof tests in an attempt to prevent cavity snouting, to reduce grout fracturing in the near cavity region, and to provide a secondary RMG stemming bulkhead in order to prevent unacceptable extrusion of the SLG
13.	MIDDLE NOTE and the subsequent events had two runs of SLG separated by a RMG column that was designed to act as a secondary stemming bulkhead. MIDNIGHT ZEPHYR and DIAMOND BEECH had a single 4-m-long run of superlean grout located on the working point side of the FAC.
14.	On MIDDLE NOTE and subsequent events the FAC, hardened-pipe-section, and surrounding very high strength groutcrete installation was designed, using crushable zones, to move approximately with the free field. Because of its higher yield, MINERAL QUARRY had 7 crushable zones rather than the 5 used on MIDDLE NOTE. The DIAMOND BEECH and MIDNIGHT ZEPHYR designs were more rigid and limited the FAC motion to about 35-percent of the free field value. The design was changed prior to MIDDLE NOTE as it was thought that the limited FAC motion may have been responsible for forcing the SLG grout to move, in mass, out of the LOS drift.
15.	The MISSION CYBER, MINERAL QUARRY, and HUNTERS TROPHY tuff strengths, as interpreted by D. Patch from the TerraTek stress versus strain data obtained on confined samples, were about 20-25 percent lower than that of the other low yield events.
16.	All sites are highly saturated and have measured air void contents of less than 2-percent
17.	The mineralogy of the n- and P-tunnel test sites differs in the percentages of the principal components (zeolite, primary and secondary silica and clays). For example, MISSION CYBER has 20-40 percent less clinoptilolite and 20-40 percent higher amorphous material content than a typical P-tunnel site. Because the testing horizon in P-tunnel is close to the "reactive horizon" (upper level of pervasive zeolitization), there are variations in mineralogy between the P-tunnel test sites. As a result, the mineralogy of the DISTANT ZENITH site is more similar to a typical N-tunnel site than to the MISSION CYBER site. Note that the HUNTERS TROPHY stemming region had a weighted average (by exposure area in the HLOS drift) of 5-10 percent smectite clay (selectively/partially argillized).

Table 1. Continued

Note	Explanation
18.	There were no events conducted wherein the stemming column was located in a region of measurably shock conditioned material. MIDNIGHT ZEPHYR, DIAMOND BEECH, and MIDDLE NOTE were conducted in a region near the old workings in N-tunnel. MISSION CYBER, DISKO ELM, MINERAL QUARRY, DISTANT ZENITH, and HUNTERS TROPHY were conducted in relatively virgin material.
19.	MISSION CYBER, DISKO ELM, and DISTANT ZENITH are located in P-tunnel where the beds are relatively flat (2-5 degrees). The other events were sited in N-tunnel where the beds dip significantly (16-21 degrees).
20.	The MIDDLE NOTE LOS drift had a constant cross-sectional area in the region between the working point and the FAC. MIDNIGHT ZEPHYR and DIAMOND BEECH had a reverse tapered section that was designed to act as a stemming constraint to prevent extrusion of grout away from the cavity. This design was discarded as it was thought that the reverse taper may act to extrude the stemming into the cavity on rebound. Subsequent to MIDDLE NOTE the drift cross-sectional area was made smaller in the working point region so that the solid angle subtended by the drift would be similar to that used in the standard yield events. In the near cavity region the DISKO ELM LOS drift cross-sectional area was about midway between that of MIDDLE NOTE and those of MISSION CYBER, MINERAL QUARRY, DISTANT ZENITH and HUNTERS TROPHY.
21.	MIDDLE NOTE had 3 faults that intersected the Main and/or Bypass drifts. DISKO ELM had 7 while DISTANT ZENITH had 14. Other events showing an open box had 3 or less fractures intersecting the drifts. The geologic structure (i.e. faults, fractures, and bedding planes) of n- and P-tunnels differ principally in their physical characteristics and degree of saturation. Faults and most fractures in N-tunnel are filled with gouge and are generally fully water-saturated, while those found in P-tunnel are open to some extent and generally dry. Again, there are variations between the P-tunnel sites as exemplified by DISTANT ZENITH which had some water-saturated fractures near the WP.
22.	On those events showing an open box the angle of the fault intersection with the drifts was between 45-90 degrees. On those events with a shaded box the fractures were sub-parallel to the drifts.
23.	The Main HLOS drift and Bypass drift layouts on MIDDLE NOTE and DISKO ELM were nearly identical. Those of DIAMOND BEECH, MISSION CYBER, DISTANT ZENITH, and HUNTERS TROPHY were somewhat different from that of MIDDLE NOTE while MIDNIGHT ZEPHYR and MINERAL QUARRY were quite different.

A few design features, thought by the first listed author to be of particular interest, are discussed in the following text. As previously indicated, recognized design differences are described in detail in Table 1.

MIDNIGHT ZEPHYR and DIAMOND BEECH were proof of concept tests. These tests examined the performance of the stemming plug, FAC, and hardened pipe section. The LOS was terminated by a witness plate that on DIAMOND BEECH was located at some distance on the portal side of the FAC. Their containment system designs were significantly different than those of subsequent tests.

MIDNIGHT ZEPHYR and DIAMOND BEECH had the FAC positioned at RS 33.5 m, a pipe taper of 1.90 cm/m, an iron extension, a 6.35 mm-thick LOS pipe, a helix whose thickness continuously increased from 6.55 mm at its point of origin at

the end of the extension to 15.9 mm where it terminated at the FAC, and a relative stiff FAC/hardened-pipe-section that limited the FAC motion to about 35% of that of the free field. In comparison, MIDDLE NOTE had the FAC positioned at RS 46.2 m, a 1.42 cm/m pipe taper, a tungsten extension, a 4.8 mm-thick pipe, a 4.8 mm-thick helix that ran between RS 23 m and RS 32 m, and a FAC/hardened-pipe-section that allowed the FAC to move with the free field ground motions.

Furthermore, the MIDNIGHT ZEPHYR and DIAMOND BEECH LOS drifts, which included a reverse taper section designed to act as a constraint to prevent extrusion of grout back into the cavity, were stemmed with rockmatching grouts to RS 32 m with superlean grout running the remaining 4 m to the FAC. Beginning with MIDDLE NOTE, the LOS drift, which had a constant cross section, was stemmed with alternating lengths of rockmatching and superlean grouts out to the FAC.

In the stemming plug formation region located between the WP and FAC the MISSION CYBER and DISKO ELM containment system designs were nearly identical with the exception that the cross-sectional area of the MISSION CYBER LOS drift was reduced in the WP region so that the solid angle subtended by the drift, at the melt radius, was equal to that of the standard yield events. The DISKO ELM as built drift cross-sectional area was about midway between that of MIDDLE NOTE and MISSION CYBER. Subsequent events were similar to MISSION CYBER.

MISSION CYBER, DISKO ELM, and DISTANT ZENITH were located in P-tunnel while the other events were sited in N-tunnel. These tunnels differ some in terms of geologic structure, mineralogy, and bedding plane dip. There are also variations in geologic structure and mineralogy within P-tunnel and as a result, the DISTANT ZENITH site is probably more similar to a typical N-tunnel site than to the MISSION CYBER site.

The geologic structure (*i.e.* faults, fractures, and bedding planes) of n- and P-tunnels differ principally in their physical characteristics and degree of saturation with those in N-tunnel filled with gouge and generally fully water-saturated and those in P-tunnel open to some extent and generally dry. However, the DISTANT ZENITH P-tunnel site has some water-saturated fractures near the WP.

The P-tunnel tuff is younger and contains more primary silica and clay than the older, strongly zeolitized tuff of N-tunnel. Because the testing horizon in P-tunnel is close to the "reactive horizon" (upper level of pervasive zeolitization) there are variations in mineralogy between sites. The mineralogy of the MISSION CYBER site indicated a lack of secondary silica (Opal-CT) and a low percentage of zeolite while the DISTANT ZENITH site was more thoroughly zeolitized and secondary silica was present. The DISKO ELM data suggest a petrographic composition intermediate to MISSION CYBER and DISTANT ZENITH.

The MINERAL QUARRY and DISTANT ZENITH design yields were larger than that of MIDDLE NOTE. However, their containment system designs were yield scaled to that of MIDDLE NOTE, MISSION CYBER, DISKO ELM, and HUNTERS TROPHY.

In summary, MIDDLE NOTE and subsequent events had more conservative containment designs (*e.g.* lower pipe taper, larger WP to FAC distance, etc.) than did the MIDNIGHT ZEPHYR and DIAMOND BEECH proof tests. With few exceptions, the six effects tests had, at least in a scaled sense, similar containment system designs in the WP to FAC/hardened-pipe-section region. Three of the low-yield effects tests were sited in P-tunnel and three in N-tunnel.

3 CONTAINMENT SYSTEM PERFORMANCE COMPARISON

Eighteen containment system performance parameters are compared on Figure 5 for the eight low-yield tests. This performance information was obtained from event diagnostics and postshot observations.

On Figure 5, an **open** box indicates the performance was as expected. A **partially shaded** box indicates the performance was somewhat different than expected while a **fully shaded** box indicates the performance was significantly different than expected. The **absence** of a box indicates that performance data was not obtained. Note that there was no GSAC, TAPS, or Test Chamber on the MIDNIGHT ZEPHYR and DIAMOND BEECH tests.

A few performance observations, thought by the first listed author to be of particular interest, are discussed in the following text. Detailed explanations of the these differences are provided in Table 2 where the item numbers correspond to the numbers appearing at the bottom of the columns found in Figure 5.

MIDNIGHT ZEPHYR and DIAMOND BEECH were proof of concept tests. Their performances differ significantly from those of subsequent tests as follows. First, there was virtually no pipe flow. Second, portions of the DIAMOND BEECH LOS pipe/helix were found that were not fully closed and a section of the pipe/helix assembly that was originally located near the extension was found in front of the FAC. Recall that these events had a larger LOS pipe taper, heavier LOS pipe, and longer and thicker helix. Third, the MJ-2A rockmatching grout in the MIDNIGHT ZEYPHR near cavity region was severely fractured while the weaker DBRM-2 rockmatching grout used on DIAMOND BEECH was less fractured. Finally, superlean grout was forced out of the LOS drift on the MIDNIGHT ZEPHYR and DIAMOND BEECH events which had relative stiff FAC/hardened-pipe-sections that limited the FAC motions to about 35% of that of the free field ground motions.

The P-tunnel ground motion data, measured on DISKO ELM and DISTANT ZENITH, differ from that obtained in N-tunnel. For example, below the 1 kb level the measured peak stress vs range data are lower than that obtained in N-tunnel.

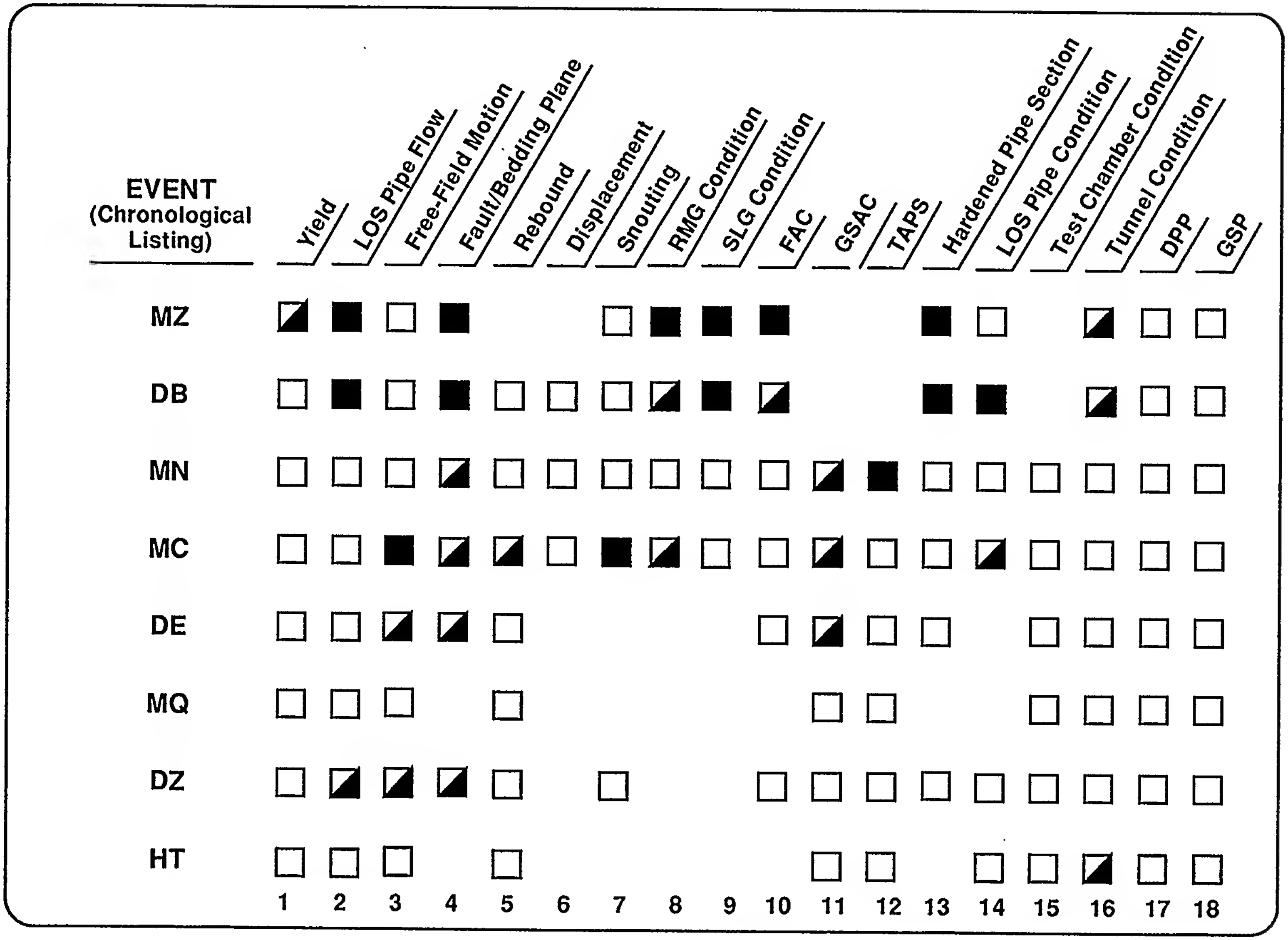

Figure 5. Comparison of the low-yield LOS event Containment System Performances.

Table 2. Containment System Performance Comparison for the Low Yield LOS Events (Explanation of notes on Figure 5).

Note	Explanation
1.	The MIDNIGHT ZEPHYR yield was slightly larger than the original design estimate but similar to that obtained on subsequent tests that used the LLNL device. Note that the detailed device performance on MIDNIGHT ZEPHYR and DIAMOND BEECH was considered optimum. The MIDDLE NOTE and MISSION CYBER performances were not optimum while that of DISKO ELM fell between these two limits. The MINERAL QUARRY and DISTANT ZENITH design yields were larger than that of MIDDLE NOTE. The actual MINERAL QUARRY/Randsburg yield was somewhat less than the design value. The HUNTERS TROPHY design yield was slightly larger than that of MIDDLE NOTE.
2.	The MIDNIGHT ZEYPHR and DIAMOND BEECH pipe flow was more benign than on the other low yield events. The stagnation pressure at the DIAMOND BEECH FAC was less than 25 percent of that on the other events. Stagnation pressure data does not exist for MIDNIGHT ZEPHYR. The amplitude and speed of the flow in the DISTANT ZENITH pipe was larger than that of MIDDLE NOTE and subsequent events.
3.	The P-tunnel ground motion data, as measured on DISKO ELM and DISTANT ZENITH differs from that of the N-tunnel events. For example, below the 1 kb level the measured peak stress vs range data are lower than that obtained in N-tunnel but similar to that predicted by calculations based on measured P-tunnel material properties. The MISSION CYBER data is unique with peak stress vs range values that are low (by at least a factor of two) compared to both the predicted response and to the data measured on other P-tunnel events.
4.	Large fault/bedding plane motions occurred in the region of the LOS drift between the working point and the FAC on MIDNIGHT ZEPHYR and DIAMOND BEECH. Smaller block motions occurred on the FAC faults on MIDDLE NOTE, MISSION CYBER, DISKO ELM, and DISTANT ZENITH.
5.	In the FAC region, rebound occurred, to within the scatter of the data, at the same scaled time and range for all events. At larger ranges, the rebound occurred earlier on MISSION CYBER.
6.	The MISSION CYBER permanent displacements were similar to those on DIAMOND BEECH and MIDDLE NOTE. Recall that the MISSION CYBER peak stress and rebound were different than those observed on other low yield events.
7.	There was no cavity snout in the direction of the HLOS drift on MISSION CYBER. It is uncertain whether a snout exists on DISKO ELM. A cavity snout was measured on those events showing an open box.
8.	On MIDNIGHT ZEPHYR the strong MJ-2A RMG in the LOS drift was severely fractured. The weaker DBRM-2 RMG used on DIAMOND BEECH was less fractured On MISSION CYBER it was found that the short RMG section located closest to the FAC had not been completely closed on axis.
9.	SLG was significantly forced out of the LOS drift on MIDNIGHT ZEPHYR and DIAMOND BEECH.
10.	A pre-closed FAC, welded to a mounting flange, was used on MIDNIGHT ZEPHYR. It was found on reentry of MIDNIGHT ZEPHYR and DIAMOND BEECH that the FACs were no longer aligned with the preshot HLOS drift direction. On MIDDLE NOTE a portion of the FAC's Cu liner was shot to the GSAC..
11.	The GSAC on DISKO ELM is the only one that formed a gas tight seal. The MIDDLE NOTE and MISSION CYBER GSACs were slightly damaged.

Table 2. Continued

Note	Explanation
12.	The MIDDLE NOTE TAPS did not close as the door was held open by a jammed uplock mechanism. The TAPS and LOS pipes, on DISKO ELM, MINERAL QUARRY and DISTANT ZENITH were sealed well enough so that on reentry the pressure on the WP side was less then atmospheric.
13.	The MIDNIGHT ZEPHYR and DIAMOND BEECH hardened pipe sections were sufficiently rigid so that the FAC displacement was limited to about 35 percent of the local free field value. This section was modified to move with the free field on subsequent tests.
14.	Portions of the DIAMOND BEECH LOS pipe that were not tightly closed were found on reentry. In addition a section of the DIAMOND BEECH pipe that was originally located near the extension was found in front of the FAC. The MISSION CYBER LOS pipe was not tightly closed in the RMG section closest to the FAC.
15.	All test chambers were in good condition.
16.	On MIDNIGHT ZEPHYR and DIAMOND BEECH, spall and floor heave occurred over large areas. Minor spall and floor heave occurred on MIDDLE NOTE, MISSION CYBER, DISKO ELM, MINERAL QUARRY, DISTANT ZENITH, and HUNTERS TROPHY with this later event showing some increased heave and spall in some areas of the Bypass drift and crosscuts.
17.	There was no pressure or temperature increase in Vessel 2 and essentially no radioactive gas seepage to Vessel 2 on any event. There was obviously no threat to the Drift Protection Plugs on these events.
18.	There was no pressure or temperature increase in Vessel 3 and no seepage of radioactive gas to Vessel 3 on any event. There was obviously no threat to the Gas Seal Plugs on these events.

However, the measured values are similar to those predicted by calculations based on measured material properties. The P-tunnel wave forms are also broader. However, displacements and, with the exception of MISSION CYBER, rebound times are similar for all events.

The MISSION CYBER ground motion was unique. The peak stress vs range data was low (by at least a factor of two) compared to both the calculated P-tunnel predictions and to measured DISKO ELM and DISTANT ZENITH P-tunnel data. In addition, the MISSION CYBER cavity was somewhat larger than predicted, the cavity had no snout along the LOS drift, the short rockmatching grout section located closest to the FAC had not been completely closed on axis, the LOS pipe was not tightly closed in this rockmatching grout section, and the rebound times were earlier at larger ranges. However, observed displacements were similar to those measured on the other events. Possible reasons for the observed MISSION CYBER ground motion, such as the mineralogy and various other site characteristics, have been examined in detail. No credible explanation has been found to date.

In summary, the limited reentry data indicate that a better stemming plug formed on MIDDLE NOTE and subsequent events that had the more conservative containment designs (*e.g.* larger WP to FAC distance, more crushable FAC/

hardened-pipe-section, alternating lengths of rockmatching and superlean grouts, smaller tunnel cross-sectional areas, lower pipe taper, and thinner LOS pipes and helices) than did the MIDNIGHT ZEPHYR and DIAMOND BEECH proof tests. Although there were measurable differences in the P-tunnel and N-tunnel ground motions, with the exception of MISSION CYBER, the observed performances of the other five effects tests were similar. Of those events that were reentered, MISSION CYBER, was unique in that it was the only event that did not show evidence of cavity snouting along the main LOS drift.

4 POSTSHOT TUNNEL COMPLEX ACTIVITY MEASUREMENTS

Postshot stemming, LOS pipe, and tunnel complex activity measurements are compared on Figure 6 for the eight low-yield tests. In all cases the seepage that did occur was very minor. There was no personnel exposure, sample recovery was not affected, and there was no loss of data or instrumentation. Note that the short solid lines shown on Figure 6 indicate no activity was measured in the samples obtained in that region.

Seepage occurred into Vessel 2 on MIDNIGHT ZEPHYR, DIAMOND BEECH, and DISKO ELM. On other events the minor seepage was limited to the stemming region, LOS pipe and/or test chamber.

There is no apparent correlation between the Vessel 1 containment performance and cavity collapse. The cavity collapse times for those events with no seepage into Vessel 2 ranged between 0.4 and 17.2 hours while collapse times for those events with seepage to Vessel 2 ranged between 0.1 and 5.2 hours.

In addition, there is no apparent correlation between containment performance and the siting of the events in P-tunnel vs N-tunnel. Postshot activity measurements indicate that even though there were measurable differences in the P-tunnel and N-tunnel ground motions, these differences did not adversely affect the Vessel 1 containment system performance.

Activity was measured in the stemming region forward (toward the WP) of the FAC on MIDNIGHT ZEPHYR, DIAMOND BEECH, and MIDDLE NOTE. Activity was also measured in holes drilled in this region on DISTANT ZENITH. MISSION CYBER was the only event reentered where activity was not measured in the stemming region forward of the FAC.

Small amounts of activity were typically measured in gas samples collected at late times (> 15 seconds) in the region between the FAC and GSAC. There was no activity measured in the 0-300 second gas samples collected in the GSAC to TAPS region on any effects test. Furthermore, there was no activity measured in this region on reentry on MIDDLE NOTE, MISSION CYBER, and HUNTERS TROPHY, although, small quantities were detected on DISKO ELM, MINERAL QUARRY and DISTANT ZENITH. The only activity detected in a test chamber was found in the 3 hour gas sample collected on MISSION CYBER.

Event (Chronological Listing	Cavity Collapse Time (hr)	Seepage to Vessel 2	← Activity → Stemming	FAC-GSAC	GSAC-TAPS	Test Chambers	LOS Drift	Vessel 2	Vessel 3
MZ	0.6	Yes	Fractured grout fwd of FAC	In gas and water	← Design did not include → These features		41.5 min	1.2 hr	—
DB	0.1	Yes	Fault, fracture planes fwd of FAC	In Gas	← Design did not include → These features		No Data	4.3 min	—
MN	0.4	No	Fwd side of FAC in RMG	—	—	—	—	—	—
MC	11.1	No	—	In Gas (in 0.75-300 sec samples)	—	0-90 sec (none) 3 hr. gas sample (yes) Reentry (none)	—	—	—
DE	5.2	Yes	No Reentry	In Gas (15 sec-300 sec samples)	0-300 sec gas samples (none) Activity in gas and water on reentry	—	3.5 hr.	4.3 hr.	—
MQ	11.4	No	No Reentry	No Recovery	0-300 sec gas samples (none) Gas & water on Reentry	—	—	—	—
DZ	5.2	No	WP side of FAC in drill holes only	0-300 sec (none) small activity on reentry	0-300 sec gas samples (none) Small activity on reentry	No test chamber or gas samples	—	—	—
HT	17.2	No	No reentry	No Reentry	0-300 sec gas samples (none) No activity on reentry	—	—	—	—

NOTE: In all cases the seepage that did occur on these events was minor.

Figure 6. Comparison of the low-yield LOS event postshot tunnel complex activity measurements.

On DISKO ELM there is limited evidence that radioactive gas seeped past the GSAC inside a timing and firing cable jacket and it is speculated that this gas eventually reached Vessel 2. An additional gas block was installed on these cables forward of the GSAC on subsequent events. Special attention was also given, on subsequent events, to all cables exiting stemming to insure they were fanned-out per design.

In summary, the containment performances of all the low-yield tests were satisfactory. Postshot activity measurements show that, with the exception of DISKO ELM, the Vessel 1 containment performances of MIDDLE NOTE and the subsequent events that had the more conservative containment designs (*e.g.* larger WP to FAC distance, more crushable FAC/hardened-pipe-section, alternating lengths of rockmatching and superlean grouts, smaller tunnel cross-sectional areas, lower pipe taper, and thinner LOS pipes and helices) were better than those of the earlier MIDNIGHT ZEPHYR and DIAMOND BEECH proof tests.

5 CONCLUSIONS

The primary conclusion to be drawn from this study is that the performance of the low-yield effects test, Vessel 1, containment system has been excellent. In all cases where seepage did occur to the HLOS pipe or Vessel 2 of the tunnel complex, the seepage was very minor. There was no posttest personnel exposure, no effect on sample recovery, no loss of data or instrumentation, and no threat to any Vessel 2 or 3 containment structure. In fact, the low-yield test Vessel 1 containment system design has proven satisfactory for a range of yields and geologic conditions.

A satisfactory stemming plug was formed on each of the six effects tests. However, there is evidence of cavity snouting along the main HLOS drift, possible fracturing of the grout in the near cavity region, and activity in the stemming column within meters of the FAC at some times. Clearly, an improved stemming grout with self healing properties similar to those observed on preliminary tests of the gypsum cement based grouts is desirable.

Successful performance of the Vessel 1 containment system during the six low-yield effects tests is due in large part to the lessons learned from the MIDNIGHT ZEPHYR and DIAMOND BEECH proof tests. Proof tests such as these are definitely necessary when developing a new containment system design. This will be particularly true in developing a design for very-low-yield effects tests since, as is well known, the cavity pressure, or threat, remains constant whereas the stemming plug length scales with yield.

Finally, it must be emphasized that a well designed containment system will not perform satisfactorily without attention to engineering and construction detail. Proper design and construction of cable gas blocks, fanouts, cooling lines, valves, stemming grouts, etc., are critical.

ACKNOWLEDGMENTS

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INITIAL CAVITY-GROWTH/PIPE-CLOSURE PHENOMENA

by

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ABSTRACT

During the initial cavity formation process the ground-motion-induced closure of the reverse cone and line-of-site (LOS) pipe results in the formation of a high density region (*e.g.* plug) that blocks the flow of cavity gas into the pipe. However, the dynamic closure process generates high energy material with the potential to flow down the LOS pipe and to thereby affect further pipe closure and/or possibly impact mechanical closures. The reverse cone is therefore designed to generate a competent plug while minimizing damaging pipe flow.

Analyses of the onset of the closure processes have to date been performed using a Lagrangian code initially and then coupling those results to the Eulerian SOIL(1) code. Results of those calculations are generally in agreement with existing pipe flow measurements to the extent they show both a first and second flow and, in most cases, they are qualitatively consistent with observed changes in flow amplitude associated with design modifications of the reverse cone and LOS pipe. However, the calculations which describe the first few milliseconds of the closure process, tend to over-estimate the plasma flow magnitudes and show no evidence of a jetted liquid flow that has at times been suspected of impacting the mechanical closures.

In an attempt to better understand the early phase of the closure process (*e.g.* possible jetting of high density material down the LOS pipe), calculations have been performed using the second-order Eulerian MESA code(2) recently developed by Los Alamos National Laboratories (LANL) to simulate 2-D and 3-D motion of multiple materials undergoing large displacements and distortions.

Comparisons of the results of the SOIL and MESA calculations are presented. Implications of the differences in the calculated results regarding continued stemming plug formation and mechanical closure performance are discussed.

1. INTRODUCTION

In typical DNA weapons effects tests a nuclear device produces a radiation pulse which shines down an LOS pipe to a test chamber which contains a

number of experiments. To protect the experiments from damage and to prevent radiation leakage into the tunnel complex, the LOS pipe is equipped with fast-acting mechanical doors and the pipe and stemming are carefully designed so that they will be squeezed closed by the ground shock as it propagates radially outward from the explosion. A schematic of the ground shock closure is shown in Figure 1.

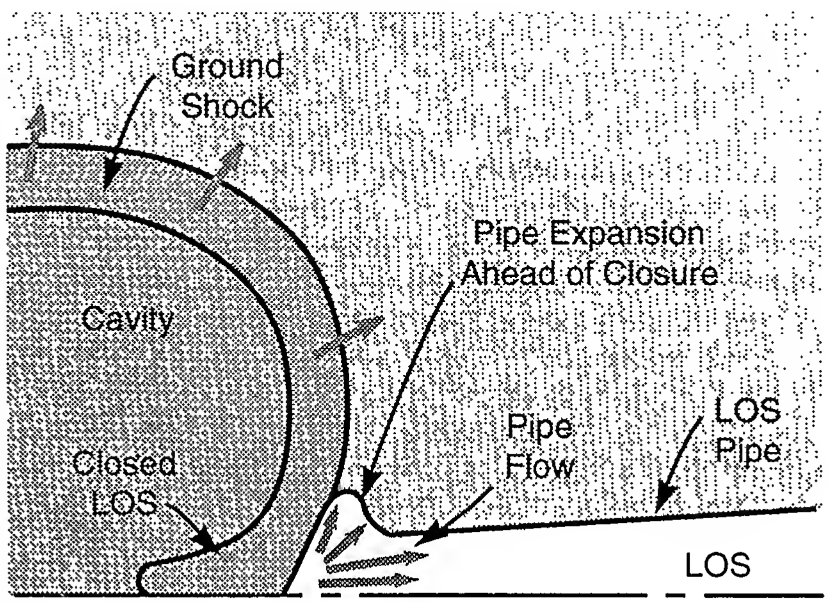


Figure 1. Schematic showing ground shock closure of LOS.

As illustrated, closure of the LOS pipe is a very complex ground-shock driven process that is coincident with dynamic growth of the cavity. The process is known to be sensitive to source region volume and energy density, extension geometry, LOS pipe size and taper, etc. Understanding this closure process becomes ever more important as DNA continues its efforts to test at lower yields which must be contained with correspondingly shorter stemming columns.

Calculational models of the pipe closure process are used to predict the performance of new test configurations and to evaluate proposed design changes (*e.g.*, early closure of the pipe and potential damage to mechanical closures). Such modeling has historically been separated into three categories, "front end", "LOS pipe flow" and "stemming". For a standard yield test, the first category focuses on early times (<1 ms), the second on intermediate times (<10 ms) while the third continues until dynamic motions within the stemming column are complete. Although historically treated separately, the three processes are obviously highly interactive. The work described here represents another step in efforts to develop a better understanding of the very early time coupled cavity-growth, pipe-flow, pipe-closure process.

The general purpose of all closure calculations is to investigate the quality of closure. A long high-density permanent closure plug is clearly beneficial. However, as the ground shock drives the pipe closed energetic material is injected ahead of the closed region. This may produce excessive gas pressure inside the pipe, causing severe pipe expansion, and jetting of solid or liquid material down the pipe toward the mechanical doors. Jetted material has been implicated in cratering damage to doors and in impairing their closure. It may have also been responsible for transporting a large chunk of iron some 60 meters along the MISTY RAIN LOS and in transporting a large piece of the close-in DIAMOND BEECH LOS pipe almost to the FAC.

Although calculational models are not capable of completely describing the complex early-time closure phenomenology, they have been successfully used to judge whether a proposed design change qualitatively increases or decreases the measured LOS plasma flow. However, existing models generally overestimate the gaseous flows measured on recent DNA HLOS events and do not predict the jetted flows of high density material that are thought to have impacted some closures.

The specific purpose of the present study is to compare pipe closure phenomenology predicted by two different calculational models, SOIL and MESA. Both codes are Eulerian, as appropriate for applications involving materials that will be highly distorted. SOIL does not contain rigorous second-order accurate differencing algorithms and is inherently more diffusive and therefore thought to possibly enhance the magnitude of the predicted gaseous flow. MESA is second order and contains a sophisticated front tracking scheme.

The body of this paper presents a series of code to code comparisons, with analytical or experimental results. The following sequence of test problems provides verification of both codes in relatively simple situations before progressing to the more complex closure problems which motivate the study;

- An ideal gas shock-tube interaction problem,
- A gas-filled cavity spherically expanding into a solid material,
- An explosively driven shock tube surrounded by sand,
- A standard LOS configuration with air, grout, and iron,
- A modified LOS configuration with air and grout, but no iron,
- A modified LOS configuration with a shortened iron extension.

Each are described in the following sections and conclusions are summarized at the end of the paper.

2. SHOCK TUBE PROBLEM

The one dimensional shock tube problem illustrated in Figure 2 serves as a preliminary test of both computer codes. The gas is presumed ideal with $\gamma=1.4$.

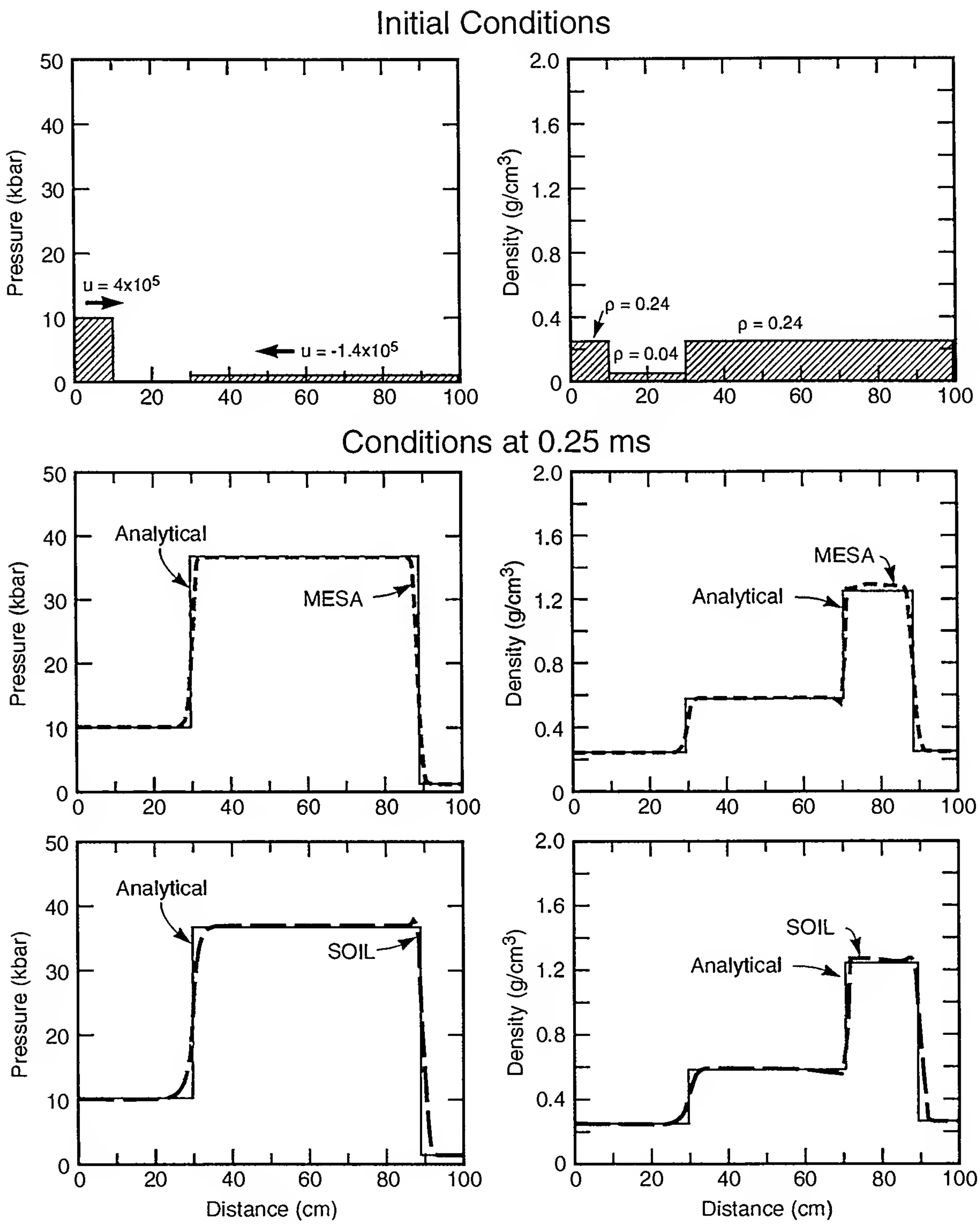


Figure 2. MESA and SOIL calculations compared to the analytic solution for the collide test problem.

The calculation is begun with two flows of equal density but differing pressure moving inward toward a central region having somewhat lower density and much lower pressure, as depicted in the upper frame of Figure 2. The subsequent collision of the converging shocks produces a flow structure having four different regions of uniform flow, as indicated by the exact analytical solution shown in the figure at a time of 0.25 ms. Up to this time, conditions have remained steady at the edges of the 100 cm computational domain. Both calculational models use 100 equally spaced zones to span this domain.

Calculational results produced by MESA and SOIL at 0.25 ms are shown on the middle and bottom of Figures 2, respectively. Both codes smear the shocks and density jumps over several zones, but the plateau values of pressure and density are in good agreement with the analytical solution. In either code it is possible to sharpen the fronts by reducing the artificial viscosity, but this causes undesirable overshoots at the edges of the jumps. For SOIL, a linear artificial viscosity coefficient of 0.3 produced the illustrated compromise between sharpness and suppression of overshoot. The MESA calculation utilized the default values of 0.0 and 2.0, respectively, for the linear and quadratic coefficients in the artificial viscosity model.

Both codes performed equally well in this simple situation, even though the finite differencing scheme used in SOIL is not formally second order accurate. In addition to the example shown here, we also ran test calculations for the more common Riemann problem having an expansion wave moving opposite to the direction of shock propagation. In summary, equally consistent results were produced by either code and there appears to be no appreciable difference between codes in these one-dimensional flows.

3. GROWTH OF A NUCLEAR CAVITY

The second test problem concerns the expansion of a spherical cavity containing a γ law gas into a surrounding elastic-perfectly plastic solid (rock) having a constant strength of 0.3 kilobar. The initial configuration is shown in Figure 3 along with the calculational results obtained by the Eulerian MESA and SOIL codes as well as the one-dimensional Lagrangian SKIPPR code[3].

The challenging feature which sets this problem apart from the earlier one is that the interface between the gas and the solid translates through the fixed Eulerian grids as the cavity grows larger. In contrast, the Lagrangian grid used in SKIPPR is attached to the elements of mass, so that the gas/solid interface is associated with a grid nodal point that can move. For this reason SKIPPR can be relied on to produce highly accurate results which serve as a standard of comparison for the Eulerian codes.

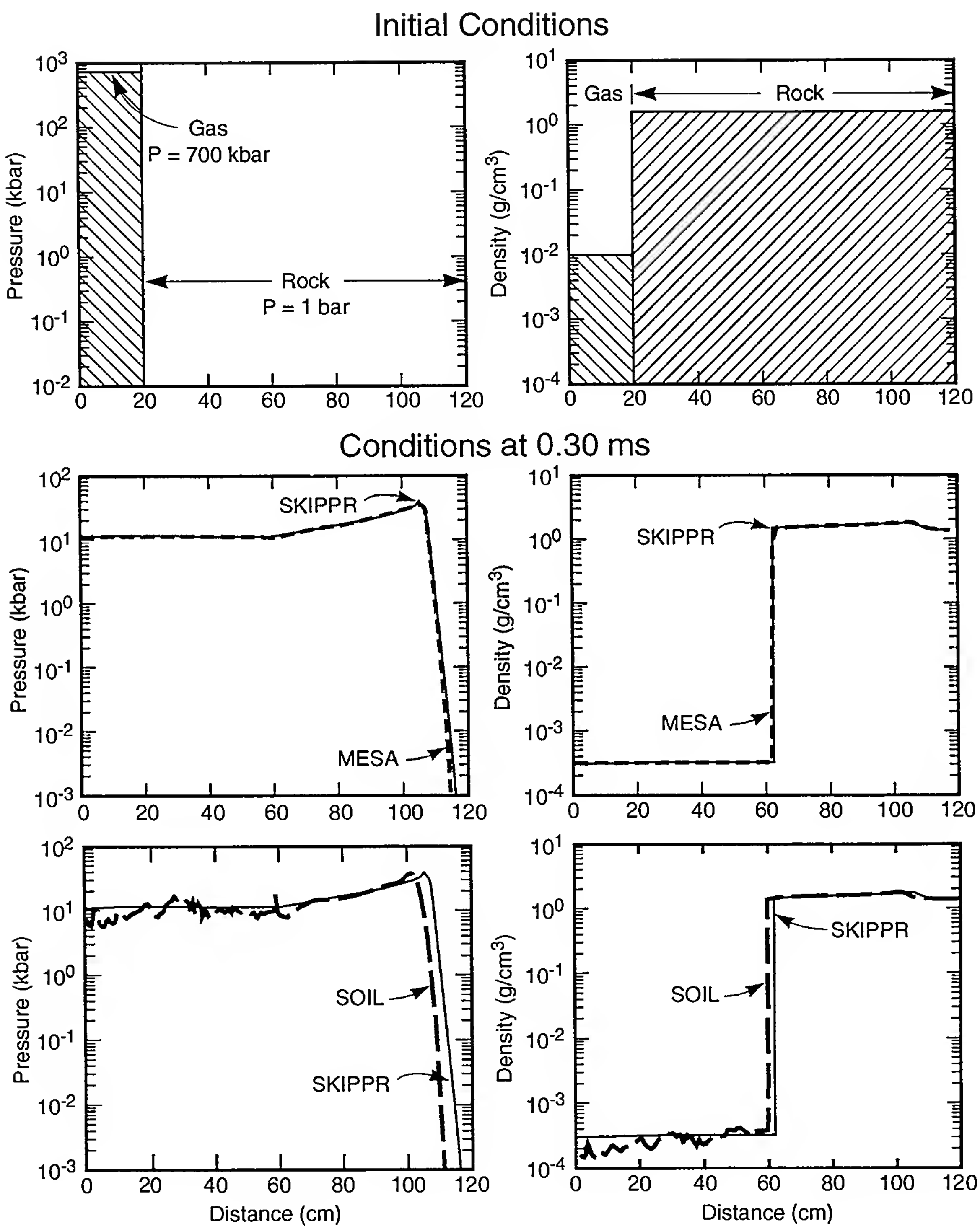


Figure 3. MESA and SOIL calculations compared to SKIPPR results for the growth of a cavity.

The treatment of material interfaces is conceptually different in MESA and SOIL. MESA tracks the position of an interface as it moves between grid lines and attempts to maintain the distinction between the material properties and the equations of state which apply on opposite sides of the interface. In contrast, SOIL uses an averaging or "mixing" algorithm to determine the thermodynamic state and the homogenized properties of any cell which contains two or more different materials. Although most mixing algorithms enforce pressure equilibrium between the mixed materials, it is not so clear how to handle thermal equilibrium or the partition of energy between materials. Also, the advective fluxes between zones are usually based on zonal averages of density and energy. These features of the mixing process result in a diffusive smearing of material interfaces.

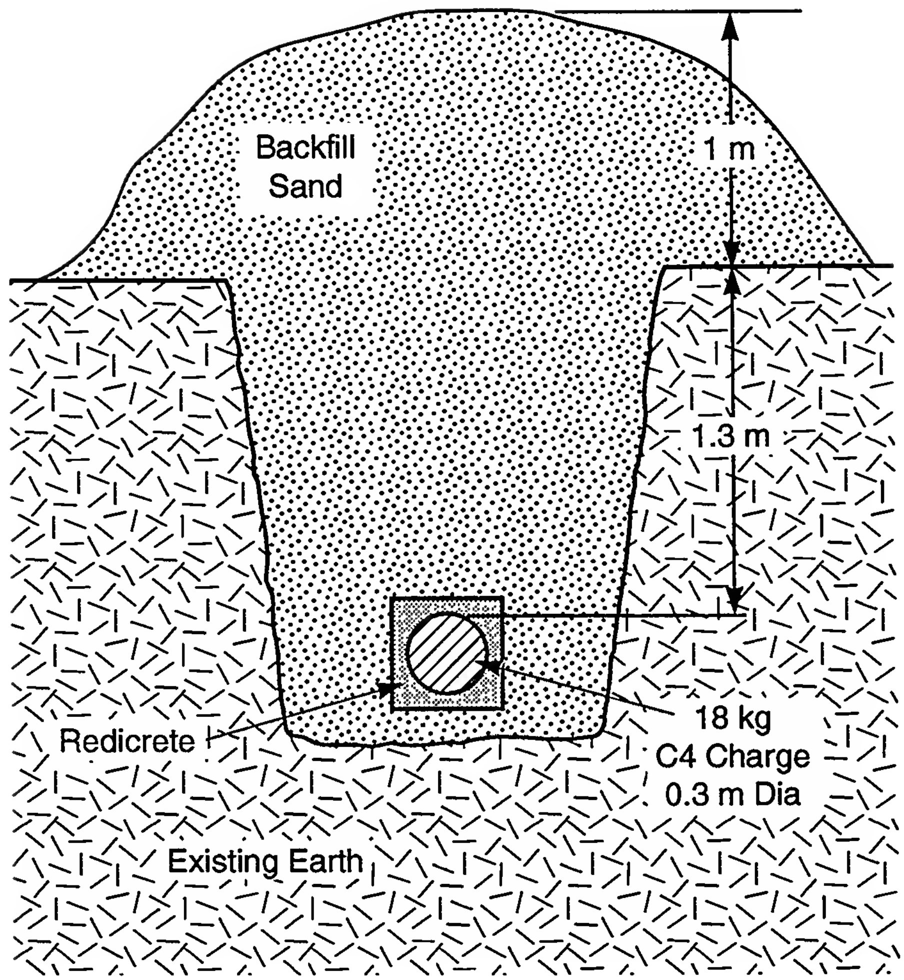
The MESA calculation very accurately preserves the density discontinuity which occurs at the cavity boundary (middle of Figure 3). In fact, for the 1 cm zoning used by MESA, the interface between gas and rock is nearly as sharp as that computed by the Lagrangian SKIPPR code. With the same zoning, SOIL does a surprisingly good job of resolving the material interface, but there are oscillations behind the shock which tend to diffuse the interface (bottom of Figure 3).

Although there is clearly some advantage to an Eulerian code, like MESA, which can preserve material interfaces, the diffusion inherent in SOIL may have a physical analog in the application of interest. Turbulent mixing of stemming grouts on scales as small as a few centimeters has been observed during reentry mining into the LOS region of previously conducted nuclear tests[4]. Since these physical mixing scales cannot be resolved by practical calculational grids, the associated process can only be modeled as a macroscopic diffusion which is not unlike the mixing which occurs in the SOIL treatment of material interfaces. In this sense, the MESA and SOIL codes may be viewed as complementary tools of analysis which, respectively, suppress and promote material mixing.

4. EXPLOSIVELY DRIVEN SHOCK TUBE

Some experimental data has recently become available for a test configuration which in many ways resembles the LOS closure problem of interest. The explosively driven shock tube shown in Figure 4 was used to investigate the response of bar gauges which were inclined at various angles to the direction of shock propagation[5]. A 0.48 cm (3/16 in) steel tube 0.3 m in diameter by 5 m long was loaded at one end with an 18 kg charge of C4 explosive and the entire assembly was buried in sand. Pressure measurements made at ranges of 2.13 m (7 ft) and 3.05 m (10 ft) from the charge are compared to the calculations.

The MESA and SOIL calculational models utilized 600 axial zones with the 480 nearest to the charge spaced 0.63 cm apart, thereafter increasing in size at a rate of 1.4% per zone. Of the 60 radial zones, 23 were spaced at 0.635 cm, 2 at



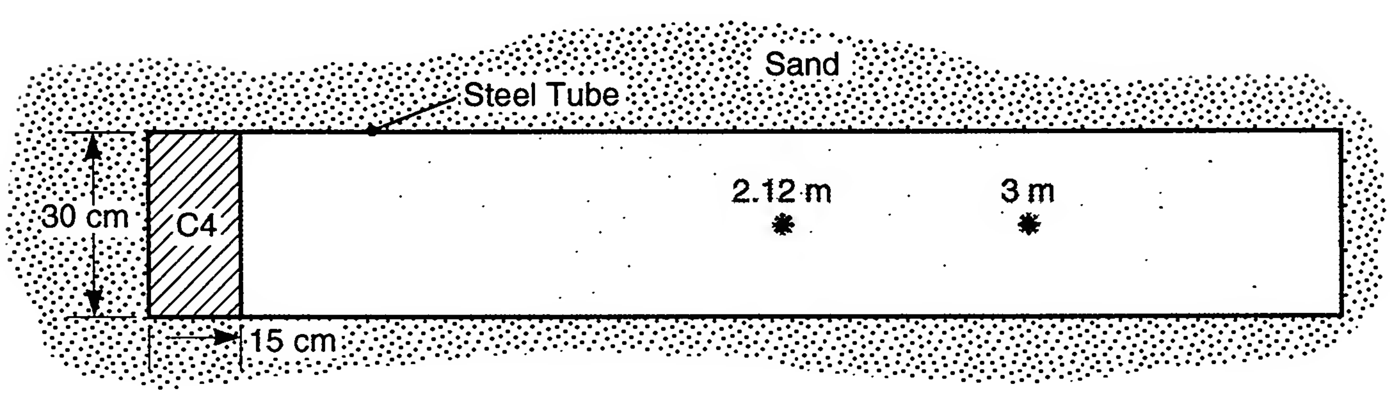


Figure 4. Experimental arrangement of the explosively driven shock tube.

0.3175, 4 at 0.12, thereafter growing at a rate of 20%. Both codes used the JWL[6] (Jones-Wilkins-Lee) equation of state for the C4 combustion products. The codes differed somewhat in their treatment of the air, sand, and steel, with MESA and SOIL using Sesame and Tillotson equations-of-state, respectively.

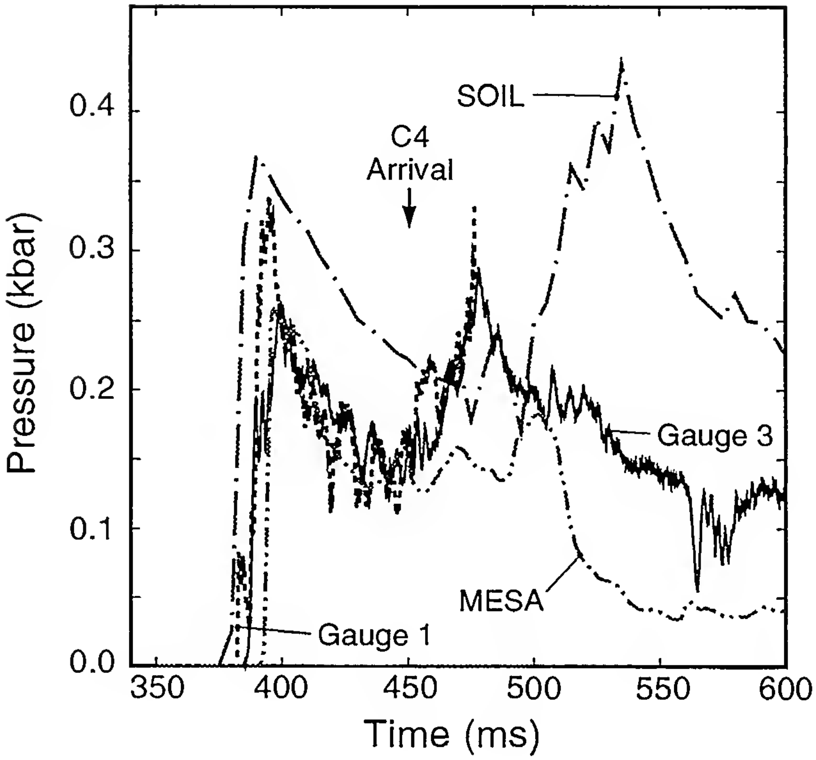
The measured and calculated pressure histories are compared in Figure 5 for the two measurement stations. All three gauges measured the radial pressure on the wall of the pipe; the two gauges at 2 m were installed on opposite sides of the pipe. The MESA and SOIL calculations generally bracket the observations. Both codes accurately predict shock arrival times within 10-15 ms (out of 380 and 565 ms), with SOIL a little early and MESA a little late. Both codes predict the peak pressures within 10-15%. The primary difference between the pressure predictions occurs in the secondary pressure rise which is much better predicted by MESA than by SOIL. At the first measurement station the secondary rise appears to correlate with the arrival of the C4 combustion products (see bold arrows in Figure 5). The pressure rise predicted by SOIL at the furthest station is excessive and comes before the apparent arrival of C4.

The pipe deformation predicted by the two codes is substantially different, as immediately apparent in the comparison at 0.35 and 0.7 ms shown in Figure 6. Note that each pair of plots shows only the left 2m of the pipe where most of the deformation occurs. Both codes indicate a dramatic expansion of the pipe in the immediate vicinity of the explosive charge. Both codes further indicate a tendency for the pipe to be closed by ground shock, but this tendency is far greater in MESA than in SOIL. MESA predicts a fully occluded pipe at 0.35 ms whereas SOIL never reduces the pipe aperture by as much as 50%. Similarly, MESA predicts an inward deformation of the pipe wall everywhere along the first meter of the pipe, whereas SOIL predicts a noticeable expansion. These same qualitative trends will also be apparent in the LOS closure simulations which are presented in the next section.

The available experimental observations do not clearly indicate which calculation provides the best prediction of the pipe deformation. A postshot photograph of the pipe shows that the end near the C4 was folded inward as indicated schematically in Figure 7. It is difficult to extend the SOIL andMESA density contours at 0.7 ms to the post-experiment observation of pipe folding.

5. STANDARD LOS CONFIGURATION

Having gained some confidence through the preceding analysis of simplified problems, we now turn our attention to the representative LOS configuration shown in Figure 8. Note in particular the massive iron reverse cone, or extension, located just outside the device chamber (Box A and B) as well as the final position of a remnant of the extension which was found on reentry of the MISTY RAIN event. This geometry is identical to that investigated by Hollis, Reed, & Wiehe[7], allowing a direct comparison of their SOIL calculations with



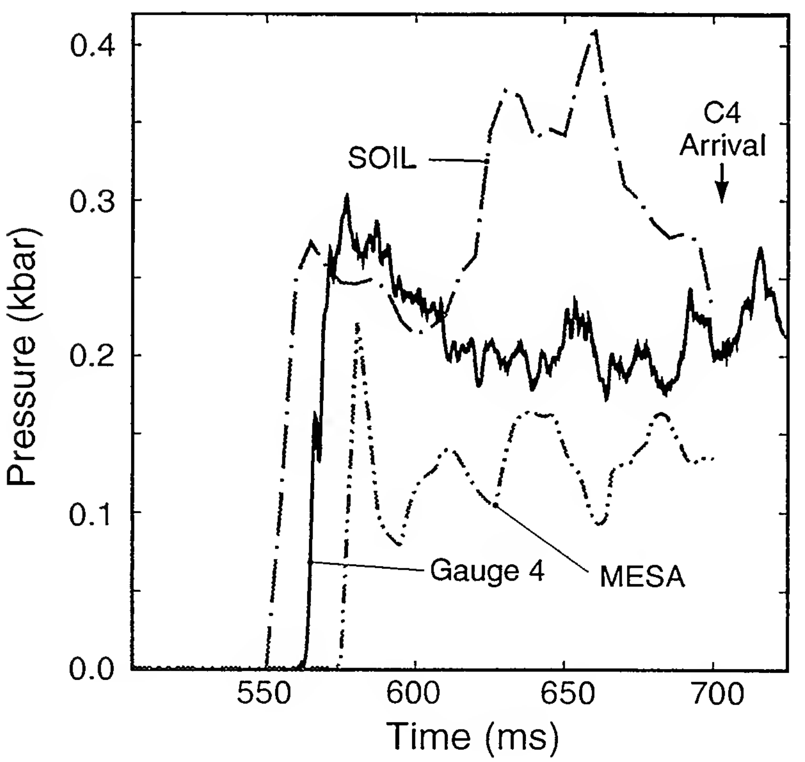


Figure 5. Measured and calculated pressure histories at 2 m (upper) and 3 m (lower) shock tube gauge locations.

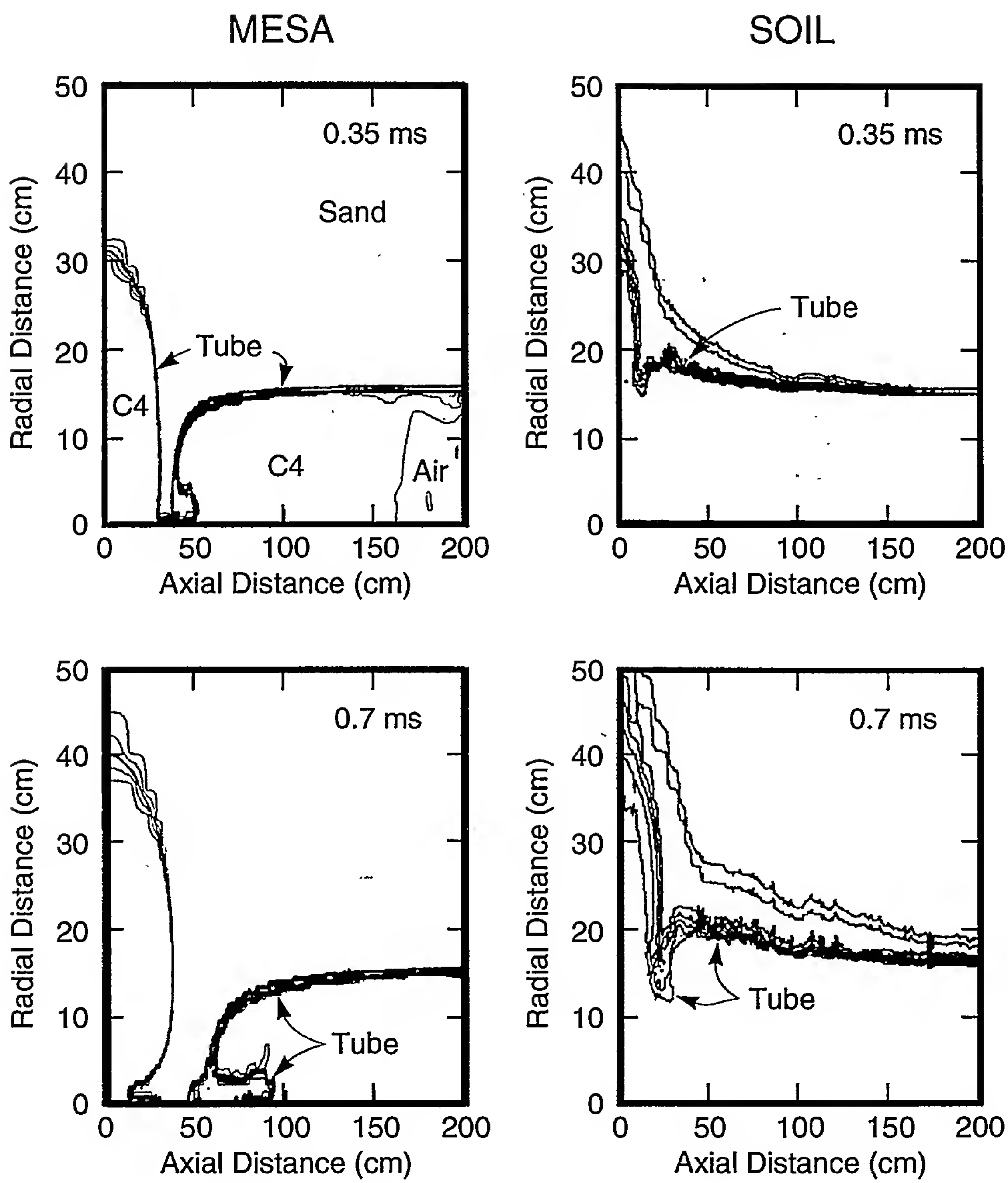


Figure 6. Explosively driven shock tube pipe deformation near the C4 charge at 0.35 and 0.7 ms from the MESA and SOIL calculations.

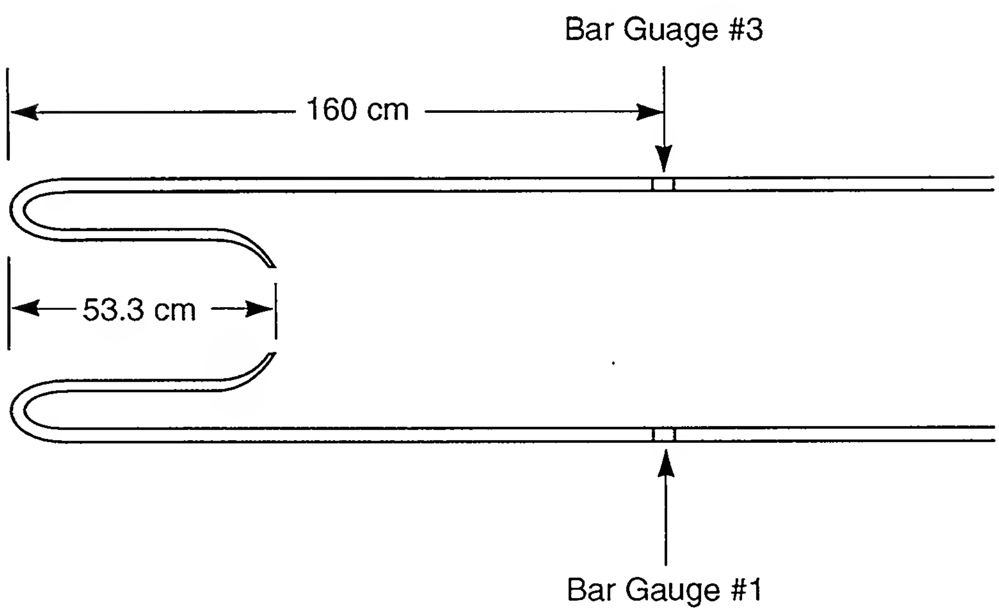


Figure 7. Measured pipe deformation in the explosively driven shock tube experiment.

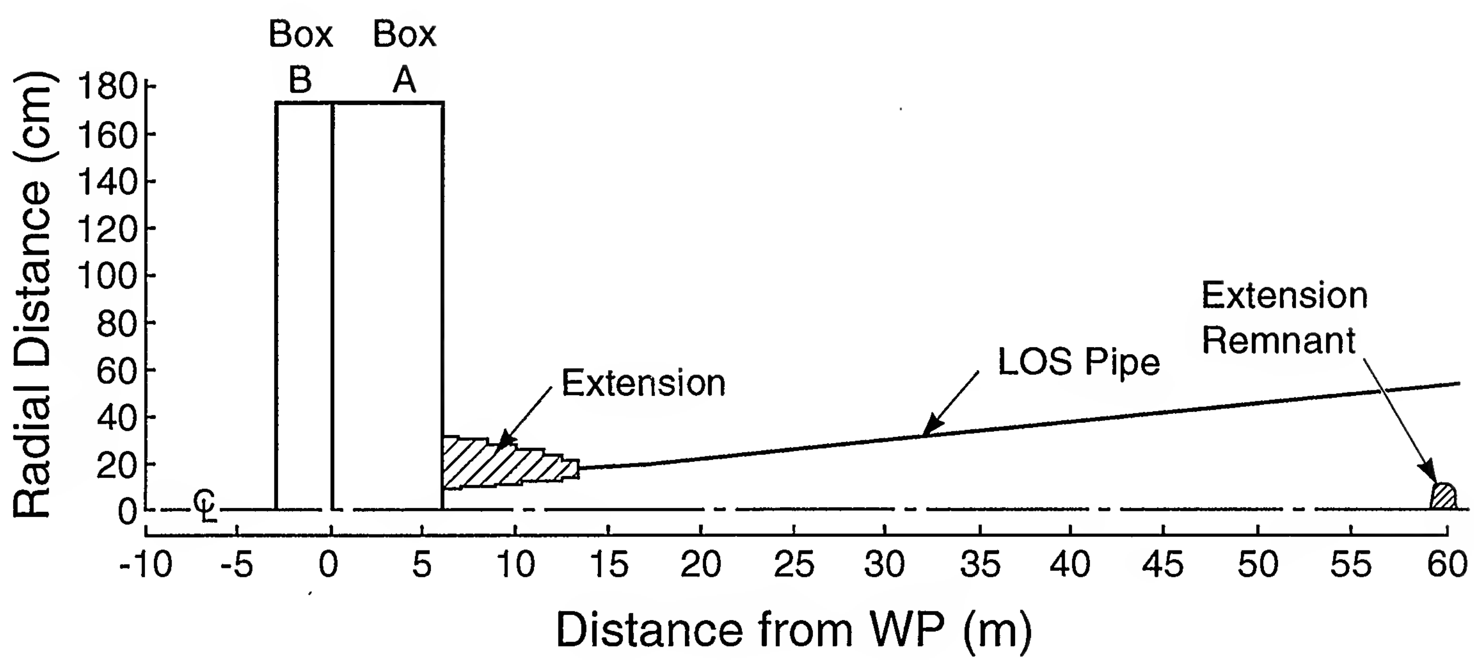


Figure 8. Representative MISTY RAIN LOS configuration showing a massive iron extension and the location of an extension remnant found on reentry.

the present MESA results. There are, however, three important differences between the two models.

- The SOIL treatment of mixed cells causes diffusion of material interfaces, as illustrated earlier in section 3.

- There is a significant difference in the equation of state used to describe the iron extension shown in Figure 8. SOIL used a modified Tillotson EOS, whereas MESA used either Sesame or Mie-Gruneisen. Although the latter two are practically the same for the region of interest here, the modified Tillotson departs from the others below 50 kilobars.

- The MESA and SOIL simulations were initialized very differently. In MESA the device energy was uniformly distributed in the total mass and volume of Boxes A and B (see Figure 8) to produce an initial pressure of 705 kilobars. In contrast, the SOIL initialization was accomplished by the procedure normally used to evaluate front end performance. It begins with a description of the nuclear source region provided by the device laboratory and detailed models of all front-end components and materials within the zero room. A Lagrangian analysis is then performed to determine the radiation deposition of the device energy and its subsequent redistribution during the first 200-300 ns. When Lagrangian cell distortion becomes extreme, the spacial distributions of mass, energy, velocities and radiation temperature are transferred to the Eulerian SOIL grid. The subsequent analysis includes the radiation diffusion approximation for as long as these effects remain important, ~ 30 μs. Beyond that time, which is short compared to the 10 ms period of interest, SOIL and MESA are solving essentially the same system of hydrodynamic equations. The more detailed model of early energy distribution leads to early energy injection into the LOS in the extension region and to higher ground shock pressure over the extension compared to the MESA simulation. The higher SOIL pressures drive the extension material much more violently into the LOS.

Other features of the two codes such as gridding, EOS models of air and grout, and elastic-plastic constitutive models are sufficiently similar that we attribute the differences in calculational outcome to the three points enumerated above.

5.1 MESA RESULTS

The evolution of the MESA simulation is illustrated by the sequence of plots in Figure 9. Each plot shows the boundaries of; the cavity, the iron extension, the cavity gas which has been injected into the LOS pipe, and the air which originally occupied the pipe. Although the LOS is evacuated, air is included in the simulations for calculational convenience. Note that there is a tendency for air to be trapped along the edges of the LOS by the tongue of cavity

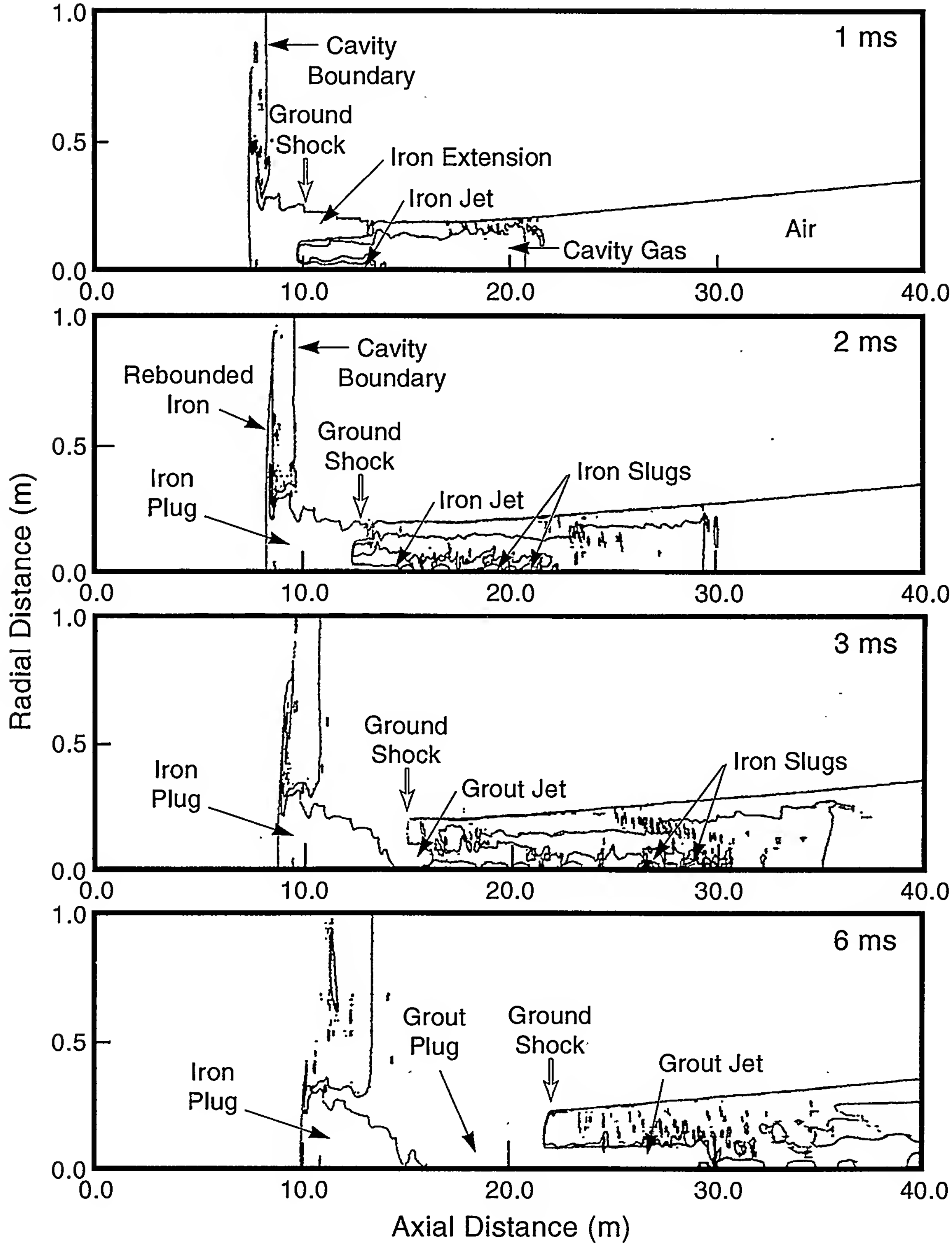


Figure 9. MESA simulation of the MISTY RAIN configuration at 1, 2, 3 and 6 ms showing material interface locations.

gas as it advances along the LOS axis. This occurs because the computational grid cannot resolve the very thin boundary layer on the pipe wall (slip-line boundaries are not yet available in MESA), but this should not significantly alter the closure process of interest. Also, the roughening and breakup of the interfaces which separate the cavity gas from both the iron and the air is probably a consequence of numerical rather than physical instabilities.

- At 1 ms, the ground shock has already traversed about half of the 7 m length of the iron extension. Some of the iron has rebounded into the cavity but a substantial fraction has been trapped in a high density plug which is ~3 m long at this time. The leading edge of the plug is coincident with the location of the ground shock, which marked by a bold arrow. Ahead of this closure point a jet of high density iron is moving along the axis at about twice the shock speed.

- At 2 ms, the closure plug has grown to a length of ~5 m, and the ground shock is just reaching the tip of the iron extension. As in the preceding frame, there is a continuous jet of iron just ahead of the closure point having a length of a few meters and a diameter of several centimeters. Further forward, the jet has begun to break up into a few slugs which are each less than a meter in length.

- At 3 ms, the shock has passed the end of the iron extension, so it is now the grout stemming which is being driven onto the axis, adding material to both the closure plug and the central jet. The grout jet has a diameter of about 20 cm, about twice that of the earlier iron jet.

- At 6 ms, the grout closure plug has grown to a length of several meters with its leading edge or closure point still nearly coincident with the ground shock. The leading edge of the grout jet is several meters ahead of the closure point, indicating that the speed of the jet is still about twice as great as the ground shock speed.

5.2 SOIL RESULTS

Results of the SOIL simulation shown in Figure 10 are reproduced from Hollis, Reed, and Wiehe. These are density contours, as opposed to the interface contours shown for MESA, but the most prominent qualitative features of the closure process are readily apparent in either type of plot. The descriptive comments which follow are a paraphrase of Hollis, et al. The comparative comments are ours.

- At 1 ms, the ground shock has just passed the tip of the extension. The shock did not reach this point until 2 ms in the MESA calculation. The faster shock propagation in SOIL is a consequence of the differences in

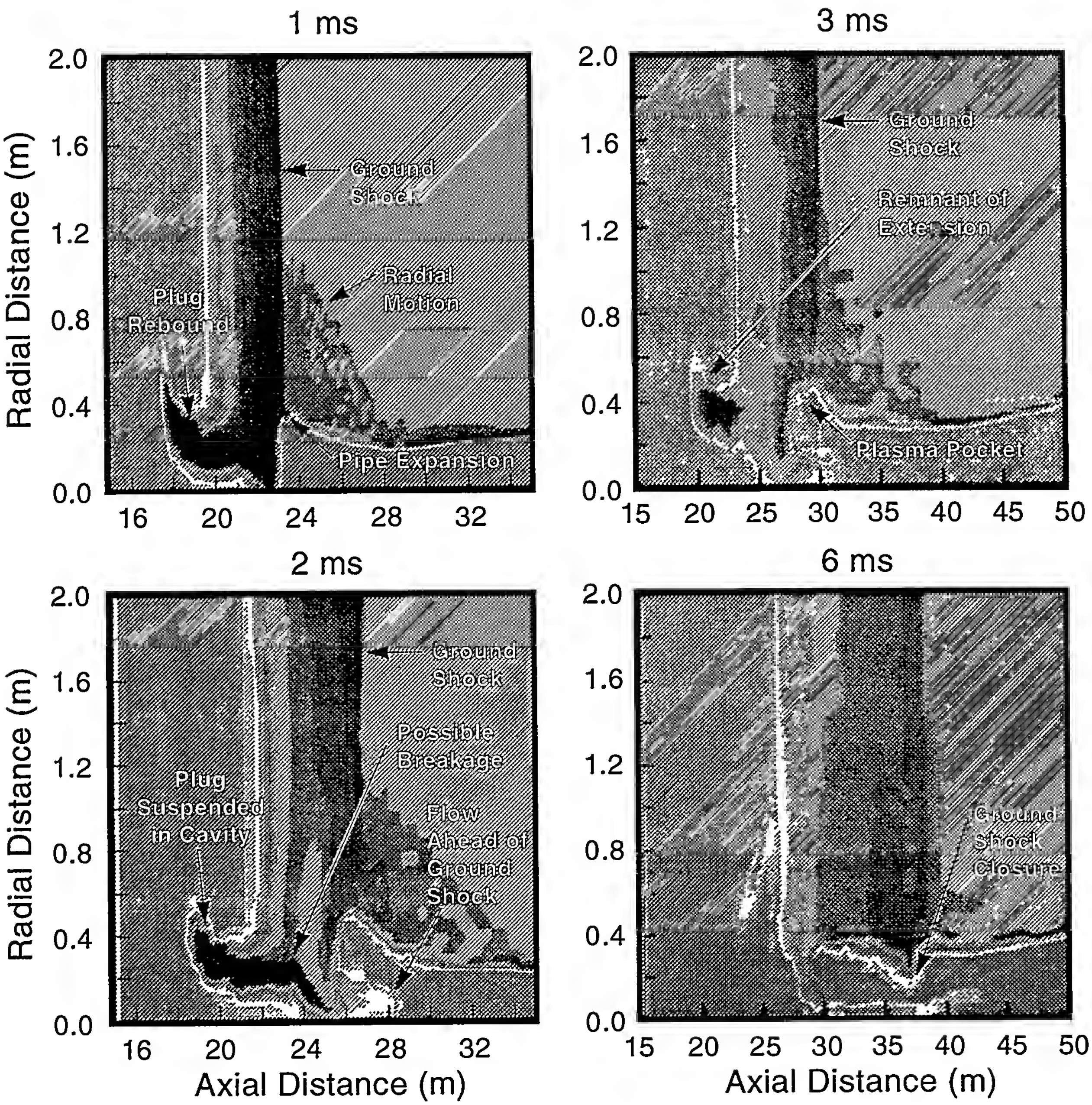


Figure 10. SOIL density contours for the MISTY RAIN configuration at 1, 2, 3 and 6 ms.

problem initialization discussed earlier. The iron LOS plug has begun to rebound into the cavity in the SOIL calculation, reducing its length compared to the longer MESA plug. This rebound of the SOIL extension may be a consequence of the greater shock pressures which drive the early phase of the closure or differences in the iron EOS used in SOIL and MESA, or both.

- At 2 ms, rebounding has lifted all but the last 1.5 m of the collapsed extension off the axis and there is some indication of a possible separation of the tip of the extension to form a slug like that observed on reentry. However, the calculated energy and density of the iron slug place it in a liquid or vapor state, rather than the solid state predicted by MESA and inferred on reentry. Similarly, the mixture of grout and iron flowing ahead of the SOIL ground shock has a density of less than unity, far less than the high-density jet of material flowing down the axis in MESA.

- At 3 ms, a "pocket of plasma" has formed just ahead of the closure point. The pipe in this region is expanded to more than twice the original diameter, as compared with the MESA calculation which showed essentially no pipe expansion ahead of the closure point.

- At 6 ms, the pocket of plasma has become more prominent than the closure plug. This situation persists, so that at the end of the SOIL calculation "there remains what is effectively a 10 cm hole in the plug as late as 10 ms", since "the density is only about 0.5 g/cm^3 up to a radius of 10 cm". It was therefore concluded that the permanent closure of the pipe must occur at some later time, beyond the 10 ms covered by the calculation. In contrast, the MESA results suggested that within 6 ms a solid closure plug with density greater than 5 g/cm^3 had reached a length of 10 m and was continuing to grow longer and is moving in the LOS at about twice of the speed of the ground shock.

5.3 COMPARISON WITH EXPERIMENTAL OBSERVATIONS

There is very little data which can be used to validate LOS closure calculations. SLIFER cables and pressure gauges provide indications of ground shock arrival and peak pressure, but the interpretation of these gauge responses is far from precise and these features are not particularly sensitive to physical features of the LOS configuration or to details of model predictions. Reentry mining provides only limited qualitative information regarding early phenomenology. It is only clear that the closure processes completely destroys the nearest section of the LOS pipe and that there is significant mixing of the various stemming grouts. In general, all of the available observations are in reasonable agreement with the predictions of either MESA or SOIL.

Figure 11 shows a comparison of pipe pressure measurements of first LOS flow (symbols) with MESA predictions of peak pressure versus range. The measured early arrival of moderate pressures is generally attributed to the flow of high energy plasma. Thus, it is not surprising that MESA would underpredict these pressures, since the device energy was uniformly distributed in all of the cavity material. Note that the predictions of peak pipe pressure are typically about twice as great as the free-field ground shock pressure owing to the collapse of the pipe and associated stagnation of material which converges radially onto the pipe axis.

The SLIFER cable records (solid and dashed lines) shown on the bottom portion of Figure 11 are quite consistent with the MESA predictions of: air shock arrival, iron jet arrival, and ground shock arrival. Near the cavity, the air shock in the pipe has sufficient amplitude to crush the SLIFERS, with SLIFER 1 reporting first, because it is only a few cm off the pipe, followed by SLIFER 2 which was emplaced further out into the grout. At more distant ranges the air shock becomes too weak to crush the cables, such that eventually the SLIFERS will only report the arrival of the ground shock. Thus, as seen in the comparison, the SLIFER responses should be bounded by the predicted arrivals of the air and ground shocks, with a transition between these bounds as the range increases. This general behavior is equally well predicted by SOIL, as presented in Hollis et. al.

Although these comparisons with pressure measurements are reassuring, they do not help to distinguish between the two calculational models, nor are they very sensitive to design changes in the front end region. This insensitivity became apparent in running calculations for the alternative configurations which are described in the next two sections.

6. IDEALIZED LOS CONFIGURATION WITH NO IRON EXTENSION

The differing phenomenology of the preceding MESA and SOIL calculations is attributed to the three differences noted earlier: numerical diffusion of interfaces, equation of state for the iron, and problem initiation. To further isolate the primary source of disagreement, an additional pair of simulations was run with the iron extension replaced by grout. Since both codes used the same EOS for all material, we expected to see closer agreement between the codes. Also, both simulations were begun with the MESA initial condition of a uniform energy distribution in Boxes A and B. Thus, it was only the first of the three considerations noted above, that of numerics, which differed between the two codes.

Figure 12 shows a comparison between the earlier MESA calculation with an iron extension (upper frame) versus the modified configuration with a grout extension having the same material properties as the surrounding material

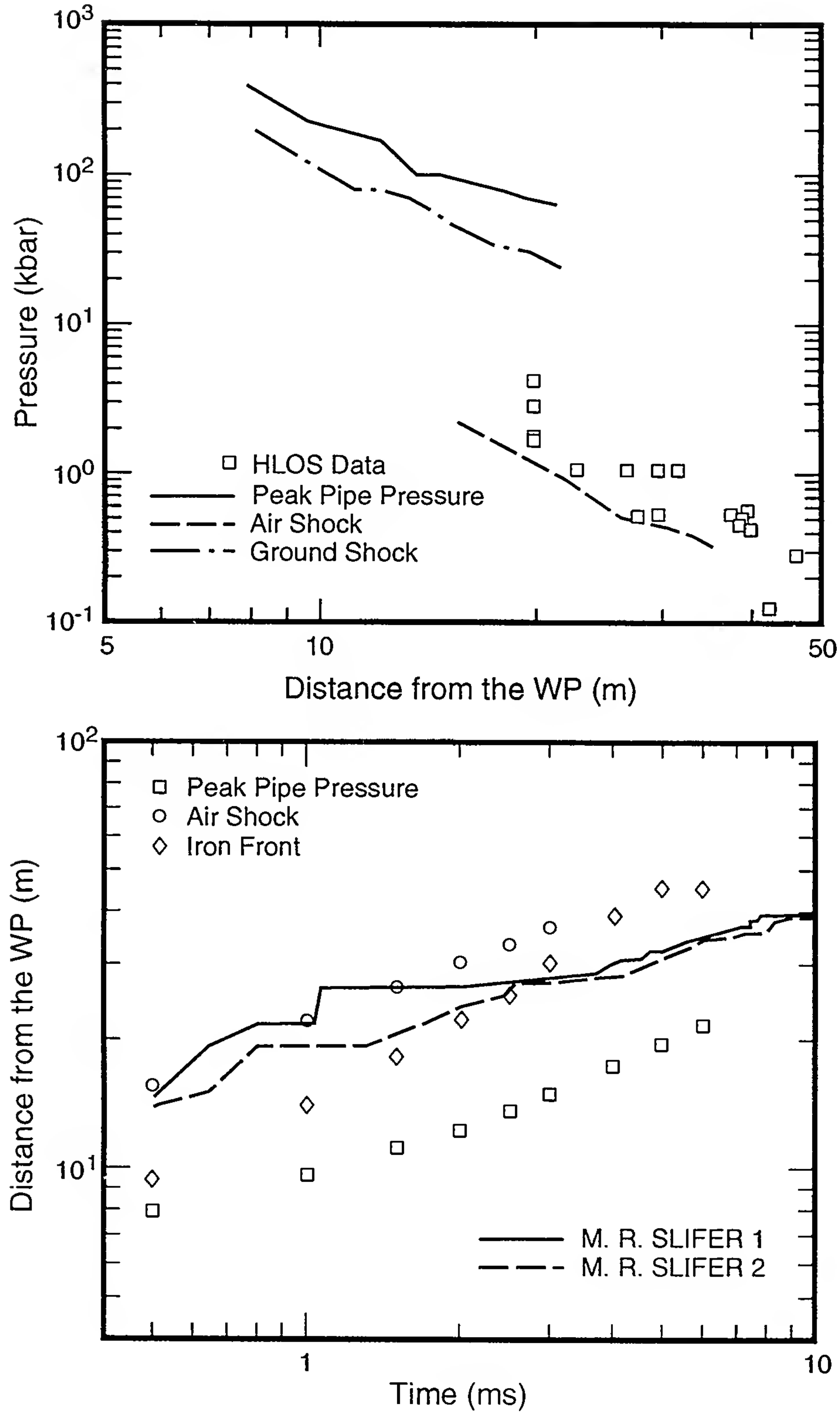


Figure 11. Comparison of pipe pressure and TOA measurements with MESA predictions for MISTY RAIN.

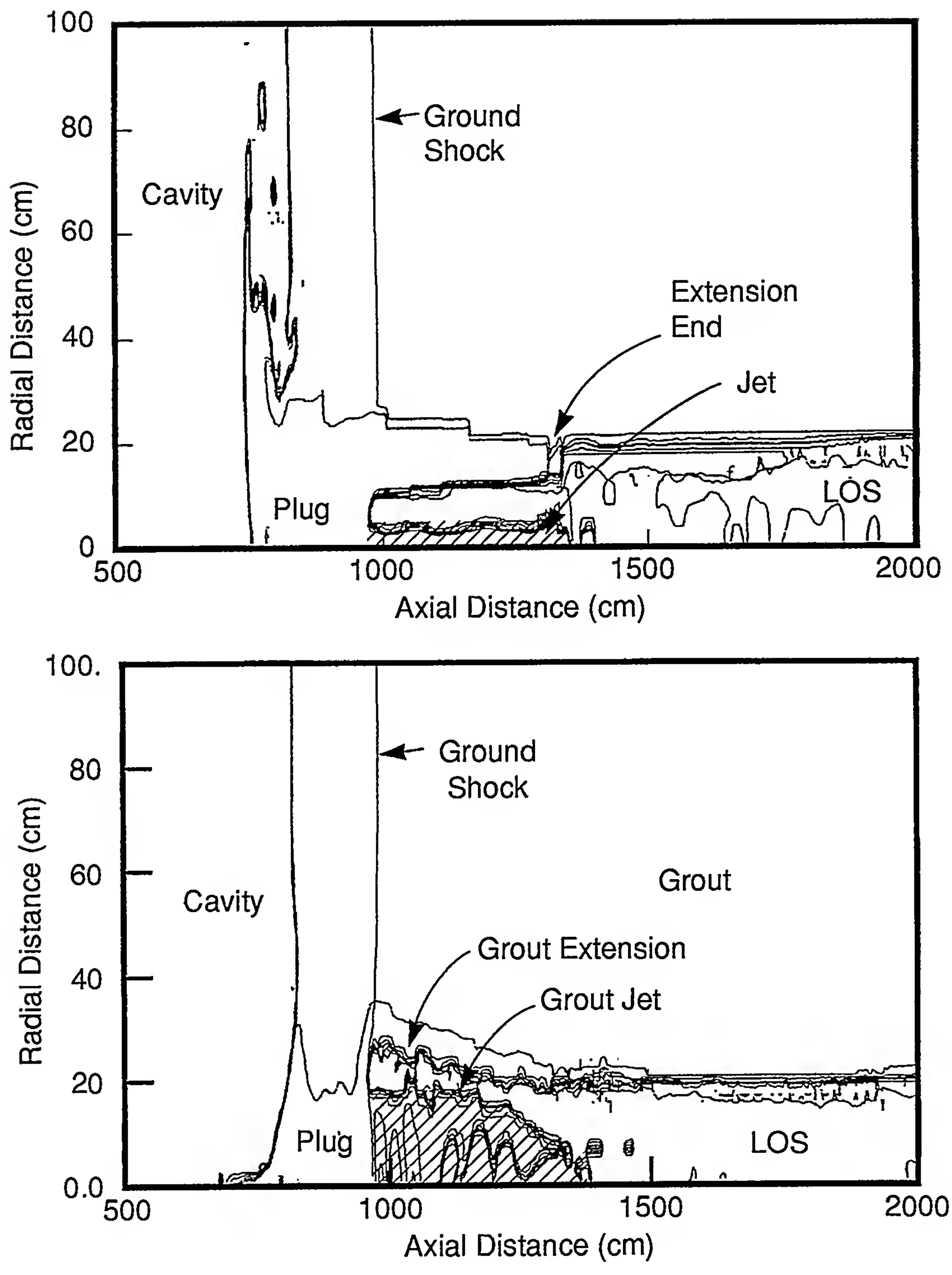


Figure 12. Comparison of MESA results at 1 ms for MISTY RAIN with iron and grout extensions.

(lower frame). Both of these MESA calculations show the formation of a high density plug in the region behind the ground shock and a jet of solid material moving forward of the closure point at twice the shock speed. However, as expected, there is considerably more pipe expansion ahead of the closure point when the iron is replaced by grout. The high density of the iron extension is intended to reduce this expansion and, hence, facilitate the closure process.

The corresponding SOIL and MESA calculations for a grout extension were in much closer agreement than in the earlier pair of calculations with iron extensions. Although SOIL still experienced much more pipe expansion than MESA, both codes predicted the formation of a high-density grout plug between the cavity wall and the ground shock closure point. SOIL and MESA density contour plots at 1 ms are shown on Figure 13.

7. MODIFIED LOS CONFIGURATION WITH SHORTENED IRON EXTENSION

Although MESA and SOIL produce qualitatively different results for the closure of an iron extension, they both appear to provide qualitatively similar guidance regarding the efficacy of proposed design changes. This preliminary conclusion is based on a MESA calculation in which the iron extension was shortened to half of the length used in the earlier MISTY RAIN simulation, see Figure 8. Also the pipe taper was decreased slightly from 0.73 cm/m to 0.63 cm/m in radius so that the geometry would resemble the DINING CAR event. However, to aid in the interpretation of results, the source description was left the same as that used for MISTY RAIN. Based on earlier SOIL analyses of similar design changes, it is expected that the shorter extension will be detrimental to the closure performance, while the difference in pipe taper is too small to be influential in the near field. These expectations are reinforced by the MESA simulation which follows.

Figure 14 compares the earlier MESA simulation for a long extension (lower frame) with the present result for a shortened extension (upper frame). At 1 ms, the shock has reached the end of the shorter extension. As before, there is a dense iron plug behind the closure point and an iron jet moving along the axis. However, the region ahead of the closure point is now showing greater expansion, as seen earlier in the case where all of the iron was replaced by grout. Thus, it would appear that the shorter extension will qualitatively produce more flow of material in the LOS pipe.

A quantitative comparison between the two calculations further supports this general observation. The shorter extension permits the injection of 25% more iron mass ahead of the closure point. Similarly, the total kinetic energy imparted to the short extension is 3.5% greater than to the much more massive full-length extension. Other features of the two calculations such as ground shock arrival

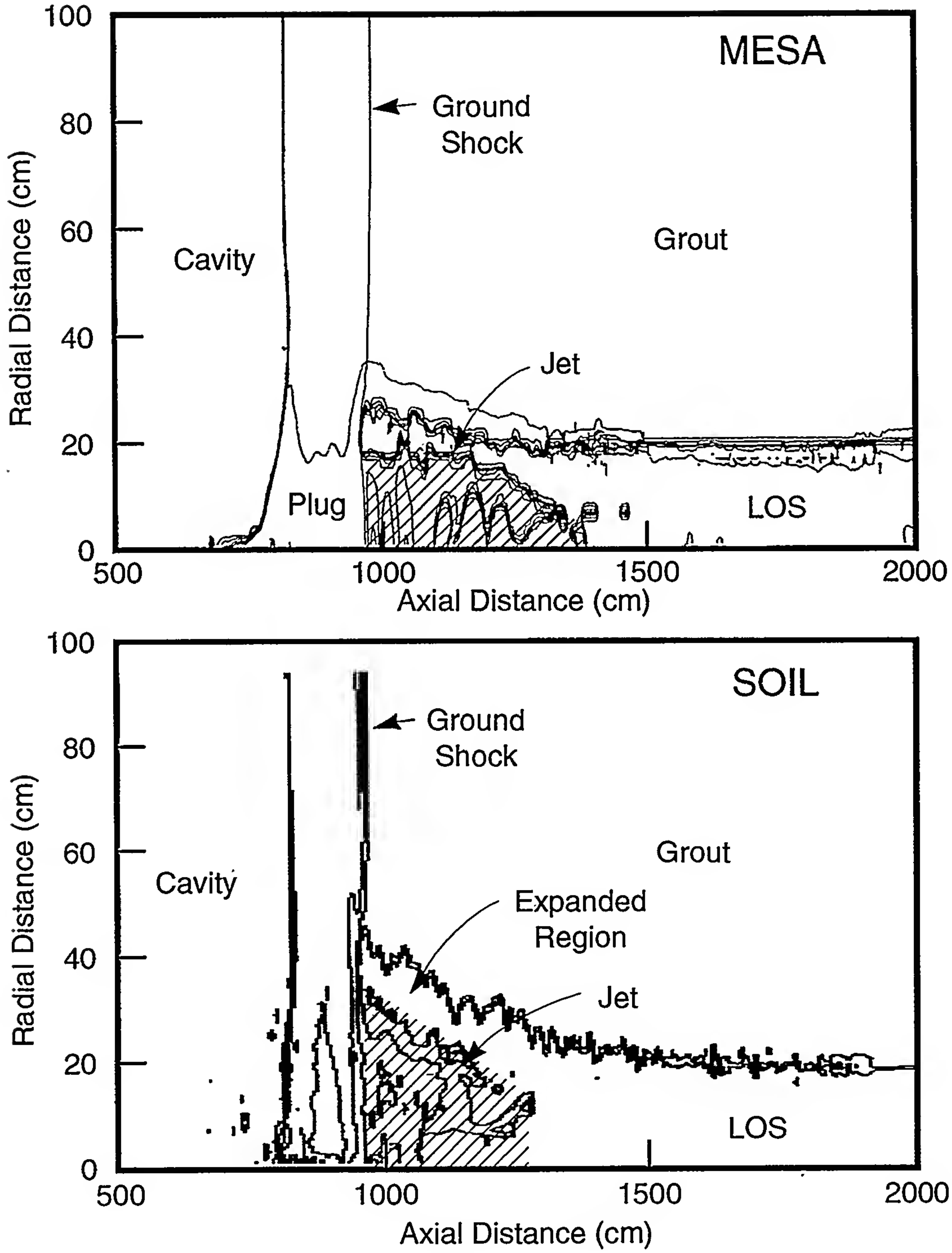


Figure 13. Comparison of MESA and SOIL simulations of the MISTY RAIN configuration at 1 ms with the iron extension replaced with grout.

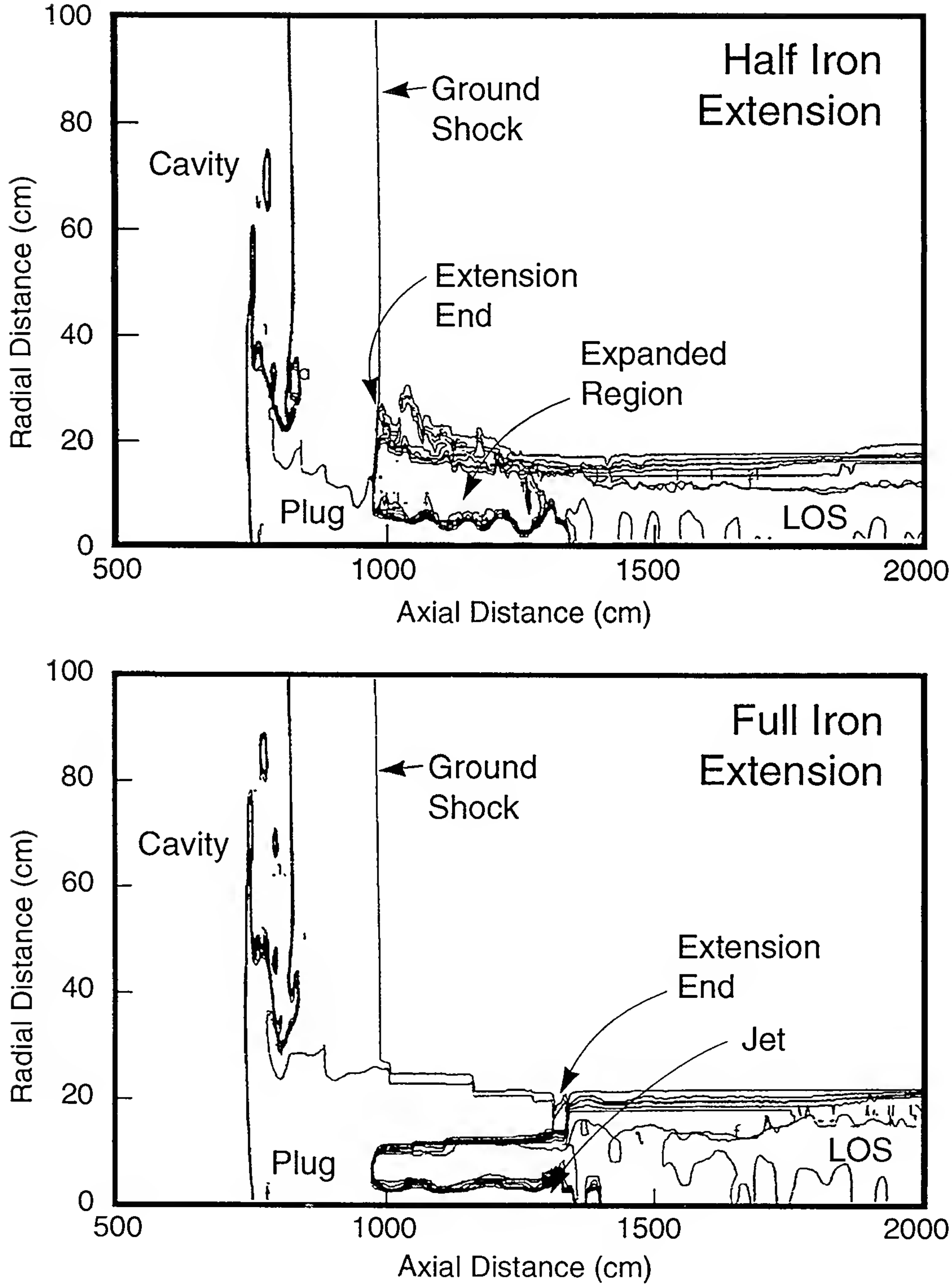


Figure 14. MESA MISTY RAIN simulation at 1 ms for a full length and half length iron extension.

and iron front arrival are very similar, though the air shock seems to arrive a little faster because of the smaller pipe taper used with the shorter extension.

8. SUMMARY AND CONCLUSIONS

The comparative performance of MESA and SOIL was first evaluated in a number of relatively simple situations where analytical solutions and experimental data are available for comparison. First, in classical shock tube applications both codes perform equally well. Second, in the cavity growth problem the SOIL treatment of mixed cells caused some smearing of the gas/ rock interface while the MESA interface remained very sharp. Finally, the experimental data from an explosively driven shock tube was well predicted by both codes, with MESA in best overall agreement with the measured pressures but SOIL somewhat more consistent with postshot observations of pipe damage. MESA predicted a tight closure of the pipe, whereas SOIL predicted only a 50% closure. This qualitative difference persisted in LOS closure predictions.

Simulations of LOS closure performance were qualitatively quite different.

- MESA predicts the formation of a permanent high-density closure plug behind the point of ground shock closure with a jet of solid iron moving ahead of the closure point at roughly twice the shock speed.

- SOIL predicts a high density closure but the iron extension rebounds off the LOS axis producing a much shorter LOS plug. Eventually a low-density hole is created through the plug that persists to the end of the simulation at ~10 ms. The pipe shows significant expansion ahead of the closure point, and the iron jetted ahead of the ground shock has very low density and high energy.

These differences in the predictions are attributed to the numerical treatment of material interfaces, the EOS used for iron, and problem initialization. Soil and MESA were in closer agreement in a simulation that used identical initialization and material properties.

Despite differences in calculational predictions, both codes provide similar guidance regarding how gross changes in the front end design relate to material flow within the LOS pipe. This was demonstrated by a test problem in which the length of the iron extension was cut in half. Mesa predicted that pipe expansion would be increased and that 25% more iron mass would be injected into the pipe. This outcome reinforces the conclusions of SOIL simulations performed a number of years ago.

Considering the great complexity of the LOS closure process it may never be possible to perform a truly accurate simulation which properly accounts for the material mixing and jetting inferred from reentry observations. These

phenomena are directly related to the violent motions which occur during the LOS collapse process, particularly in the immediate vicinity of the closure point where an unknown quantity of high energy material is trapped in the collapsing pipe. In the face of these uncertainties, the MESA and SOIL codes may be viewed as complementary tools of analysis which, respectively, suppress and promote material mixing, thus providing bounds on the expected behavior.

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DO ANALYTIC MODELS OF NEAR SOURCE PIPE FLOW COMPARE TO MEASUREMENT?

by

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ABSTRACT

For most DNA events Sandia National Laboratory has fielded pressure and time-of-arrival (TOA) measurements along the line-of-sight (LOS) pipe and in the surrounding stemming. In recent years useful data return has increased dramatically. As this technology has progressed the ability to make measurements closer and closer to the source has become feasible. The principal measurement instruments include; SLIFER cables to record shock TOA versus position, ported pressure gauges installed on the LOS pipe wall to measure the dynamic pressure of energetic LOS plasma flow and piezioelectric stress gauges embedded in the stemming outside the LOS to measure strong shock pressures in the hundreds of kilobar range.

The data base collected by Sandia scientists serves to advance our understanding of shock induced motions and energy injection mechanisms in the working point region. The long standing practice of studying the complex dynamics near an explosion using large, complex, but perfectly axisymmetric, continuum mechanics codes can be augmented by the measurement data base. Although great strides have been made in computational tools and analytic methods, the present models do not address "real" geometries, inhomogeneities or "real" materials. One such numerical challenge is to describe the evolution and generation of LOS plasma and its continuous interaction with the LOS pipe and the surrounding stemmed region. The Sandia measurements represent a look at reality and allow us to determine shortcomings in the numerical modeling approach.

A collection of measured and calculated observables is presented and compared with particular attention given to data gathered on recent events. Unexpected results are noted, calculational inadequacies are discussed and lingering puzzles are presented.

1. INTRODUCTION

For a number of the most recent DNA horizontal line-of-sight (HLOS) events a suite of shock and pipe flow measurement data has been collected. Data from the nearly identical DISKO ELM (DE) and HUNTERS TROPHY (HT) events has been chosen for comparison with computer calculations that are customarily used to evaluate Working Point (WP) region phenomenology. The purpose of this comparison is to illuminate potential areas of code modification and enhancement to improve the accuracy of the numerical simulations. This study is a continuation of a series of ongoing comparisons

between measurement and analytic predictions. Previous investigations have been reported at past Containment Symposia [Ref 1, 2, 3, 4, 5].

The following section includes a brief description of the phenomenology associated with LOS pipe flow and ground shock closure of the LOS pipe. In the next section a discussion of the analytic methods used to model the environment is presented followed by an overview of the type and location of pertinent gauges fielded on the DE and HT events. The final two sections contain a comparison of measurement and predictions followed by a summary with some recommendations for possible code improvements and future work.

2. PHENOMENOLOGY

A discussion of LOS pipe flow and LOS pipe closure can most easily be presented with the aid of the cartoon shown in Figure 1 depicting the conditions near the WP, also known as the Front-End (FE) region. For reference, the time frame for this cartoon is maybe 100 microseconds after nuclear energy release. By that time a strong

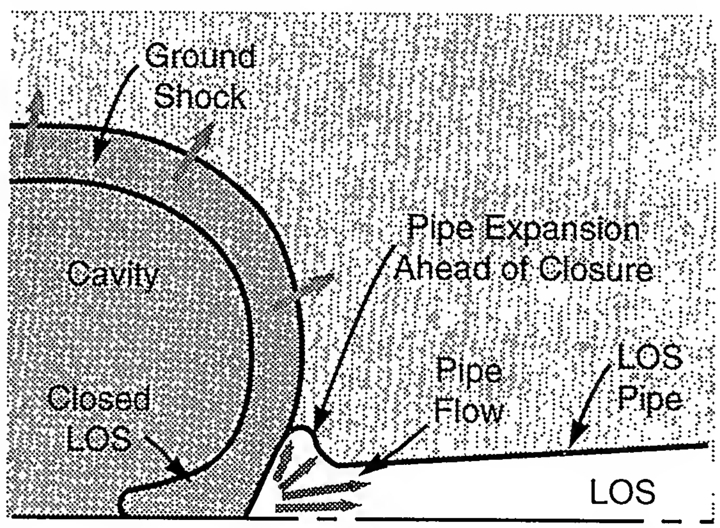

Figure 1. Cartoon showing ground shock generated LOS pipe closure near a growing cavity.

ground shock has propagated away from the growing cavity into the stemming material surrounding the LOS pipe. The ground shock is of sufficient strength to easily swage the LOS pipe onto the LOS pipe centerline. In fact, in the closure process low density stemming, pipe material and a FE feature just outside the device canister, known as the extension, can be squirted into the LOS ahead of the advancing ground shock. The initial part of this flow, usually called LOS plasma, carries with it sufficient pressure and kinetic energy to expand the LOS ahead of the advancing LOS closure. The latter LOS flow created by the closure process possibly includes high density material and is thought to have produced the craters observed in the doors of mechanical closures and to have at times impeded door closure operation. This is the phenomenology that we attempt to describe with our analytic tools, which are discussed next.

3. ANALYTIC TOOLS AND METHODS

To accurately model the pipe flow and LOS closure depicted in Figure 1 the analyst begins with a source description provided by the device laboratory and calculates the redistribution of energy within the device canister and enclosed materials. This is done most accurately with a Lagrangian computer code in which the computational mesh is attached to elements of mass. The Lagrangian code used at S-CUBED is a derivative of the LUDWIG[6] code that describes radiation flow in the diffusion approximation and includes a hydrodynamic description of material motion. As material motions intensify the Lagrangian mesh tangles and the calculational time step becomes overly restrictive. The Lagrangian simulation typically ends in the 0.25 to 0.5 microsecond time frame. This analytic model is continued with the Lagrangian results being transferred to the two dimensional SOIL Eulerian code[7]. The SOIL calculational mesh is fixed and as time advances materials move through the mesh subject to mass, momentum and energy conservation. The SOIL code continues the radiation diffusion and hydrodynamic treatments with the addition of an elastic-perfectly-plastic material strength formulation. The strength treatment becomes important when stresses drop to a level near the yield strength of a material.

The LUDWIG-SOIL analytic approach is used to evaluate FE designs, to obtain an estimate of LOS pipe flow and to determine the sensitivity of FE operation and pipe flow resulting from FE design changes. An accurate description of LOS closure, plasma generation and plasma interactions with the LOS pipe are extremely difficult to address computationally. The most difficult phenomenology to model is the interaction of the low density, high velocity, energetic plasma with the LOS pipe. These interactions determine the amount of material that can impact mechanical closures and hinder their operation. A more accurate approach would require a combined Lagangian-Eulerian (ALE) formulation that would allow 'slip' between the fast moving plasma and the essentially stationary LOS pipe. Also, turbulance and ablation must be carefully coupled to the hydrodynamic formalism. A single computer code having these complex interactive features does not exist. However, there are approximations, such as pipe wall ablation models[8] available in SOIL that can be used in conjunction with pipe flow data to improve the analytical predictions. The comparison of SOIL predictions and measurement, discussed in the next section, is crucial in the proper application of such models.

4. DISKO ELM AND HUNTERS TROPHY GAUGE TYPES AND LOCATIONS

DE and HT events used identical devices and their FE and LOS designs were nearly replicated. Also, LOS pipe flow data was collected on these events using the same gauge types located in similar locations. A sketch indicating gauge types and locations relative to the WP, LOS helix and FAC is shown in Figure 2. The following gauges were selected for comparison with analytic prediction. On the HT event two Yetterbium stress gauges (Yb) were fielded about 2 cm outside the LOS pipe in the stemming relatively close to the WP at 8.6 and 13.93 m. On DE three SLIFER cables, located 0.3 and 0.6 m radially from the LOS pipe, were fielded in the stemming to

measure shock Time Of Arrival (TOA) for signals generated by the LOS plasma and the ground shock emanating from the cavity. Fluid Couple Plate (FCP) gauges mounted flush with the inside of the LOS pipe wall were located on the WP and portal sides of the LOS pipe internal helix and on the WP side of the FAC on both events to measure LOS plasma pressure levels. Both DE and HT data will be compared for the FCP on the WP side of the helix, and DE results will be compared for those beyond the helix in the next section.

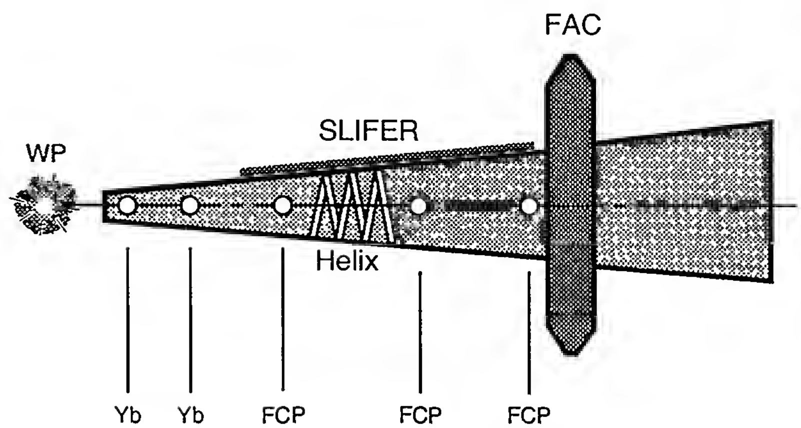

Figure 2. A sketch of Yb, FCP and SLIFER locations fielded on the DISKO ELM and HUNTERS TROPHY events.

5. EXPERIMENT AND ANALYTIC COMPARISON

Measured and calculated results for the two Yb gauges fielded just outside the LOS pipe at 8.6 and 13.93 m are compared in Figure 3. The results are confusing. First, the closest gauge reading and prediction do not agree. The gauge response occurs much earlier, 0.075 versus 0.25 ms, and the pressure level is about 2.5 times larger than predicted. Secondly, just 5.3 m further downstream the measured and calculated TOA comparison is in better agreement, 0.22 versus 0.35 ms, but the measured pressure is an order of magnitude smaller than measured at the closer-in gauge. The calculated value of pressure at the further-out gauge agrees well with measurement. The first arrival at the closest gauge seems unrealistically early to be caused by LOS plasma. The early record may be the result of radiation deposition on the LOS pipe wall But the pressure is too high for a location 2 cm into the stemming. If caused by radiation deposition one would expect that an identical gauge located only 5 m away would respond in a similar way. The discrepancy requires further study.

Figure 4 compares measured and predicted TOA data for both the SLIFER and FCP gauges. The three SLIFER records shown correspond to on-the-pipe, 0.3 and 0.6 m radial locations counting from top to bottom, respectively. The two dashed curves are calculated predictions for the on-the-pipe and free-field SLIFER locations. The on-the-pipe comparison is initially in agreement but the prediction displays earlier arrival past 13 m from the WP, indicating a more energetic early pipe flow in the calculation. The

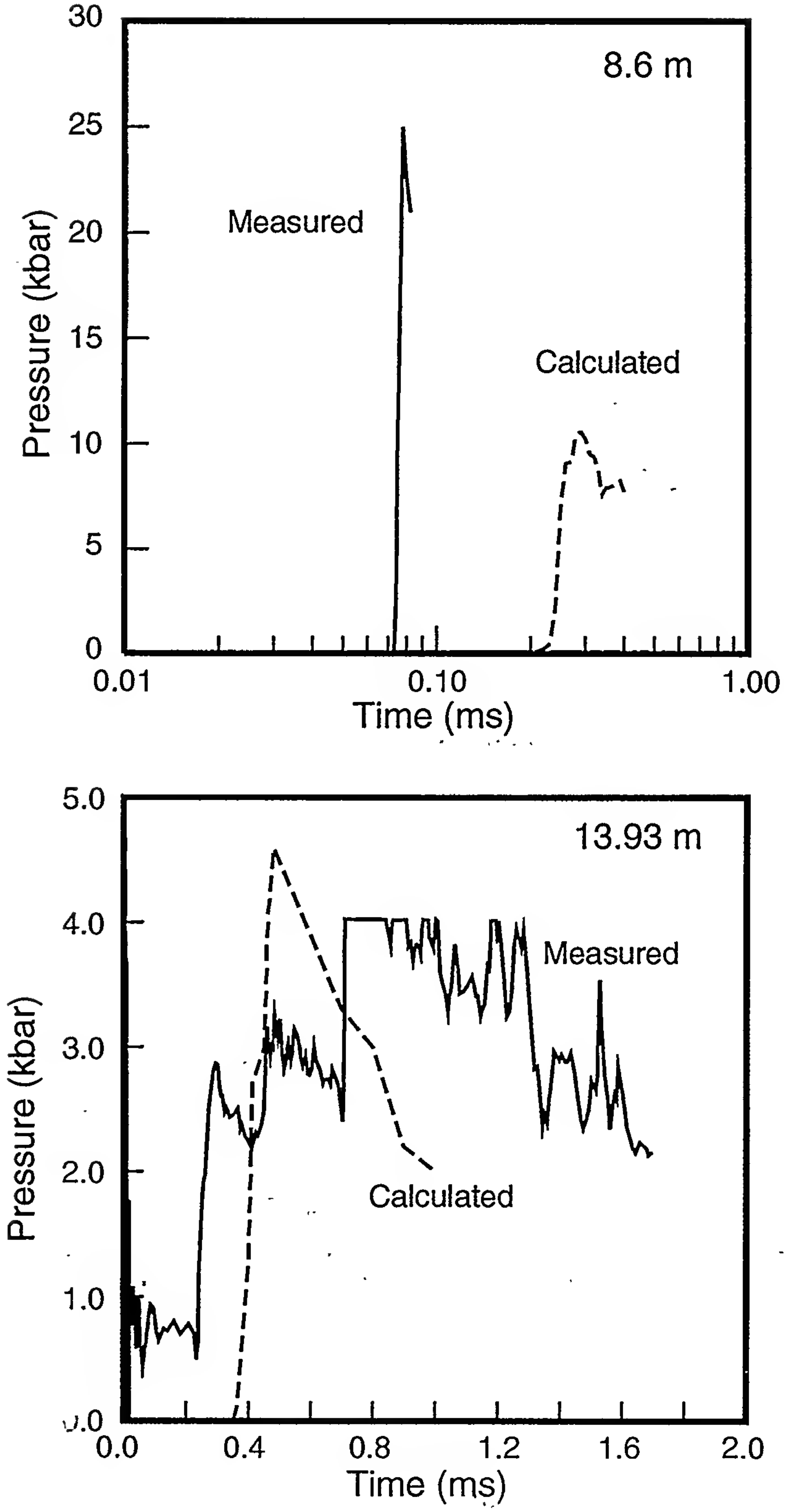

Figure 3. Comparison of HUNTERS TROPHY close-in Yetterbium gauge records and calculated predictions.

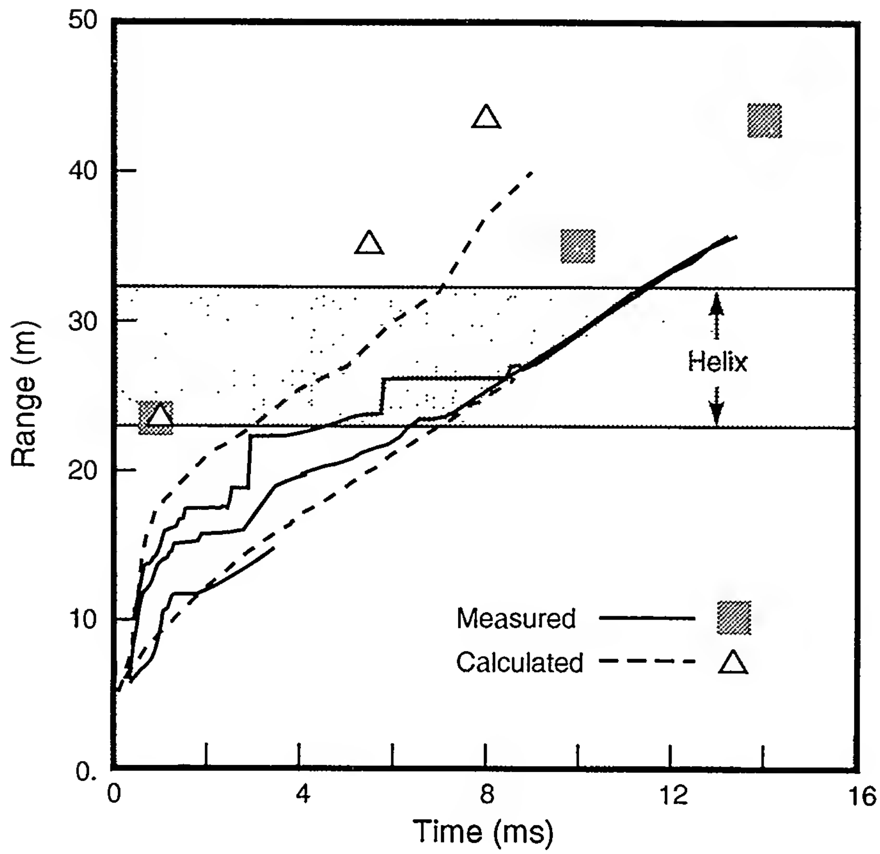

Figure 4. Comparison of measured and calculated TOA for FCP locations and on-the-pipe (upper), 0.3 m (middle) and 0.6 m (lower) SLIFER cables.

prediction and measurement for the SLIFER located at a larger radial range from the pipe are in good agreement especially at further distances from the WP where influences of shocks from the LOS flow would be too weak to crush the SLIFER until the ground shock arrives. The FCP gauge TOA data, solid squares on Figure 4, and the SOIL predictions at the same distances from the WP do not agree except for the TOA at the closest gauge at the WP entrance to the helix. SOIL predicts a higher velocity LOS flow as evidenced by the early arrival at the 34.1 and 42.5 m FCP locations on the portal side of the helix. These differences are not surprising since in the SOIL simulation there was no attempt made to model the helix or its associated slowing of the LOS flow. These analytic predictions are consistant with measurements made by SNLA (Bass et. al.) on previous events that did not have an internal helix installed inside the LOS. Ablation models in SOIL could be adjusted to replicate the helix effect.

FCP pressure records for gauges located on the WP side of the DE and HT helices, about 22 m from the WP, are compared to predictions in Figure 5. SOIL consistently predicts slightly slower flow in the LOS. The agreement in pressure values are reasonable. The oscillatory nature of the measured flow may be flow related or a function of gauge operation, but is not replicated in the calculations. Note that the measured and predicted TOA is a little earlier for HT as compared to DE. This result

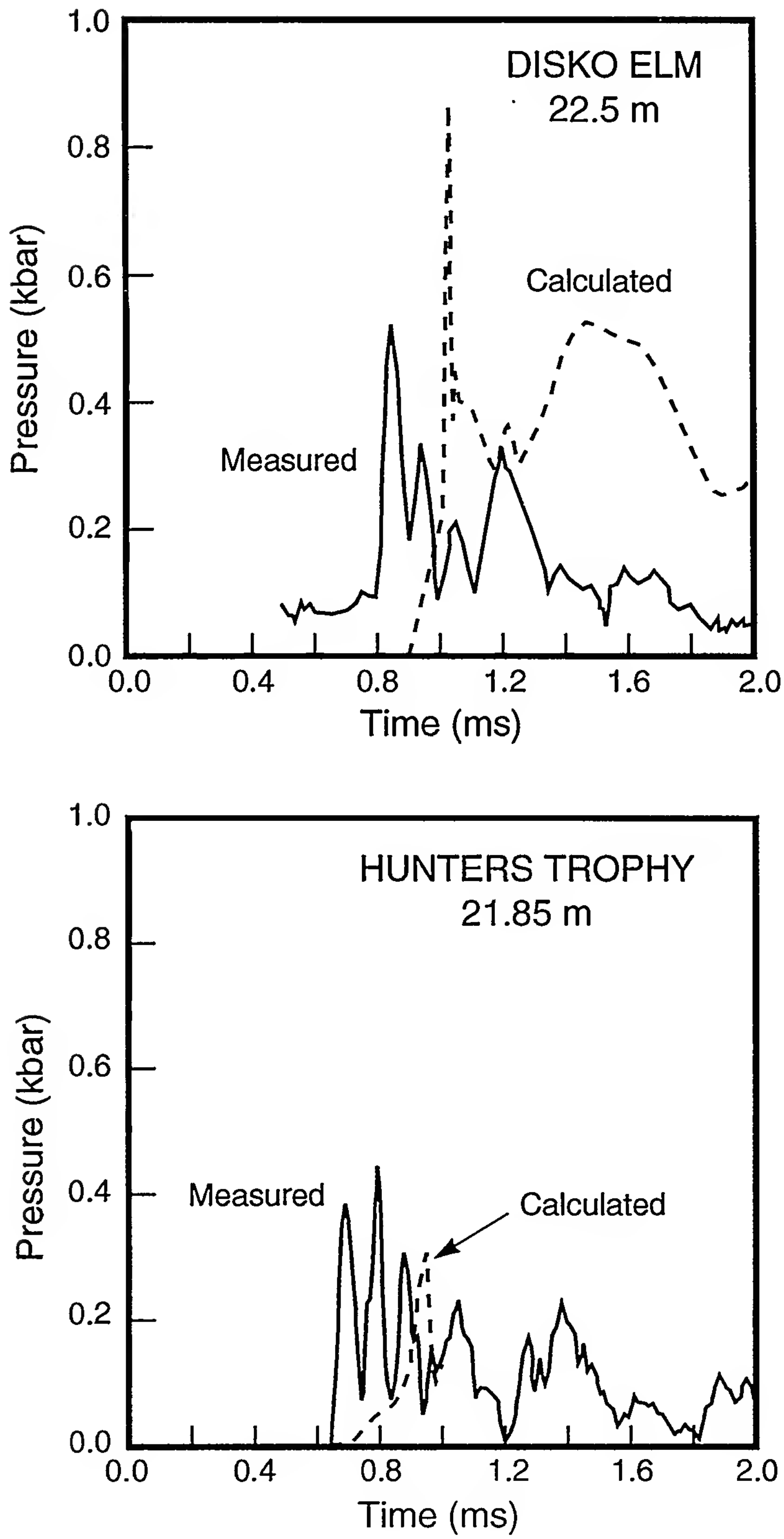

Figure 5. Measured and calculated FCP pressure records on the WP side of the DISKO ELM and HUNTERS TROPHY helices.

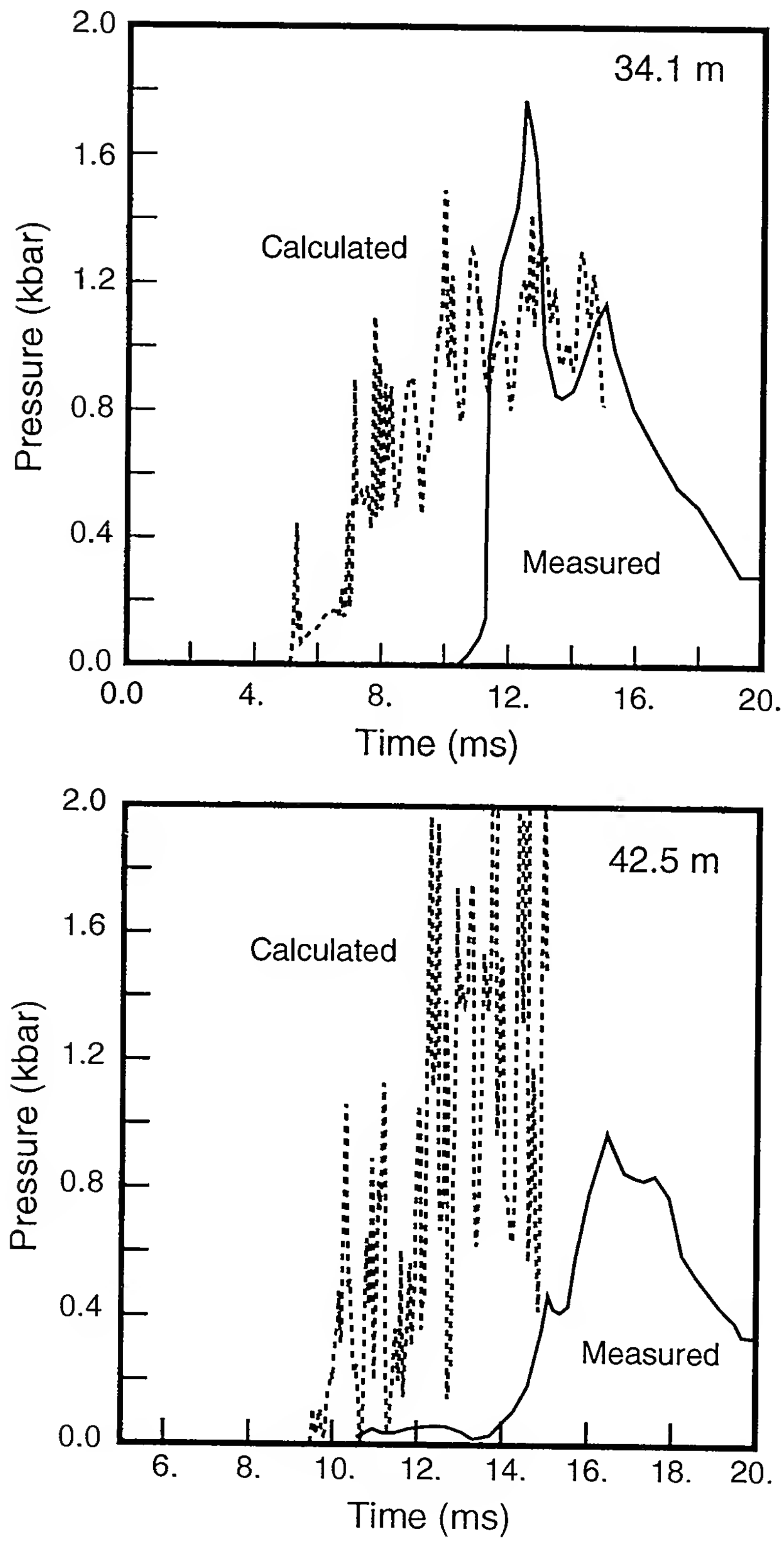

Figure 6. Measured and calculated DISKO ELM FCP pressures for gauges located on the portal side of the helix (upper) and in front of the FAC (lower).

was not a surprise because a design modification to the HT extension just outside the device canister was expected to increase LOS flow velocities. The SOIL ablation model can be adjusted in the smooth pipe section between the FE and the FAC to improve the SOIL prediction.

FCP measurements obtained on the DE event between the helix and the FAC at 34.1 and 42.5 m are compared to SOIL results in Figure 6. Since the helix was not included in the SOIL simulation it is reasonable that the code predicts much earlier TOA at those ranges. The magnitude of the measured and calculated pressure are in good agreement. The noisy calculated results are due to rather coarse zone sizes used at that range from the WP region, recalling that these calculations investigate FE and WP region behavior, and are not specifically done to determine pipe flow at large ranges from the WP.

6. SUMMARY AND RECOMMENDATIONS

In general the agreement between measurement and calculation is believed to be rather good, considering that the simulations focus on WP region phenomenology. Glaring discrepancies have been noted and reasonable explanations for them have been given. Three avenues should be taken if more accurate predictions of LOS pipe flow is required in the future. First, calculations should be repeated for the DE and HT configurations with more appropriate zoning techniques applied in the region between the FE and gauge locations. Secondly, the pipe flow results from the recalculation should be compared to measurement and the SOIL ablation model parameters adjusted in the smooth pipe regions and at the helix location until the TOA and pressure magnitudes are brought into closer agreement. Thirdly, state-of-the-art computer codes capable of modeling FE behavior and accurately model LOS interactions should be sought out and applied to typical underground test configurations.

The pursuit of the program just outlined may be in jeopardy given the changing mission of the DNA.

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The Second Pipe Flow in the Line-of-Sight Pipe

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Abstract

Explosion-generated flow in a horizontal line-of-sight (LOS) pipe is of distinctive concern to the containment community because it has been and can be a threat to the closures. We have evaluated the data from measurements on pipe flow in Defense Nuclear Agency sponsored, low-yield events of the past 17 years. Most of the measurements have been made using the fluid-coupled plate gauge, with the force-plate installed flush with the interior surface of the pipe so as to measure the side-on pressure. The data set is limited to measurements in front of the muffler, if any, and away from the closed FAC (fast-acting closure).

Our present interest is in the second pipe flow, which originates in the crushing of the LOS pipe by ground shock. This causes jetting of material from the pipe and surrounding grout. A summary of these data will be presented. We find that the peak pressures of most events, as a function of yield and range, are well-fitted by a simple formula. The outliers will be discussed. In particular, the internal helix is seen to be effective in reducing the threat to closures. Similar information on the first pipe flow has already been reported. We briefly review this flow, adding data from recent events.

CLOSE-IN SHOCK WAVE DIAGNOSTICS ON MIDDLE NOTE AND MISSION CYBER

by

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ABSTRACT

There has long been interest in testing and confirming the calculations used to design closure systems for DNA line-of-sight (LOS) experiments. Since the regime of relevance of these calculations is one of extreme conditions, conducting meaningful experiments is very difficult and quite expensive. As a result, relatively little data exists describing the initial stemming plug formation process.

Complex arrays of CORRTEX and SLIFER cables were emplaced on MIDDLE NOTE and MISSION CYBER near the Box-A, reverse cone, and LOS pipe. These data were intended to provide heavily redundant measurements of close-in shock propagation at several angles from the vertical and to thereby provide measurements that would constitute a limited test of calculations of the initial reverse-cone/pipe-closure process. A preliminary analysis of some of the data available is presented.

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* Consultant

1.0 INTRODUCTION

There has long been interest in testing and hopefully confirming the calculations used to design prompt closure systems for DNA line-of-sight experiments. The regime of relevance of these calculations is one of extreme conditions, and conducting meaningful experiments there is very difficult and quite expensive. As a result, relatively little has successfully been done.

LANL emplaced an array of CORRTEX cables on the TOMME/MIDNIGHT ZEPHER event which seemed to provide new and encouraging data as summarized in the Containment Summary Report of this event, [*Ref.* 1]. A CORRTEX cable was run from the vicinity of Box-A parallel to the LOS. Several cable loops were run down to the vicinity of the LOS and then back up to the main cable line. These "down loops" were installed perpendicular to the LOS. One of them provided data near box A which showed a propagation velocity of the complex signal from box A and the reverse cone to be much higher than predicted.[1] Since the CORRTEX system presumably provided a relatively non-controversial measure of shock propagation, it appeared to be an excellent candidate for a measurement of the shock propagation field in the vicinity of the working point. A good measurement of this field would provide a strong test of the design calculations used in the DNA LOS events. Sandia was also confident that their SLIFER system could give good shock arrival data in the vicinity of the WP.

As a result, an experiment was suggested in which a complex array of CORRTEX and SLIFER cables would be emplaced near Box-A and the reverse cone of MIDDLE NOTE to provide a heavily redundant measurement of close-in shock propagation at several angles from the vertical. We hoped this experiment would provide reliable measurements that would constitute a restrictive test of early-time calculations.

This suggestion was accepted with some enthusiasm by LANL and Sandia, and after some design iteration it led to an extensive array of diagnostic cables installed in the MIDDLE NOTE tunnel as shown in Figure 1. A photograph of this impressive array was chosen as the cover for proceedings of the 5th Containment Symposium.

The experiment was repeated on the subsequent MISSION CYBER event. Minor changes in the experimental configuration were made based on limited, preliminary analysis of the MIDDLE NOTE data.

This report will describe the experimental design and present a preliminary analysis of some of the data. More work is presently being done at LANL. They

[1] As pointed out in Reference 1, the experimentors subsequently expressed some reservations about their original written report of their work.

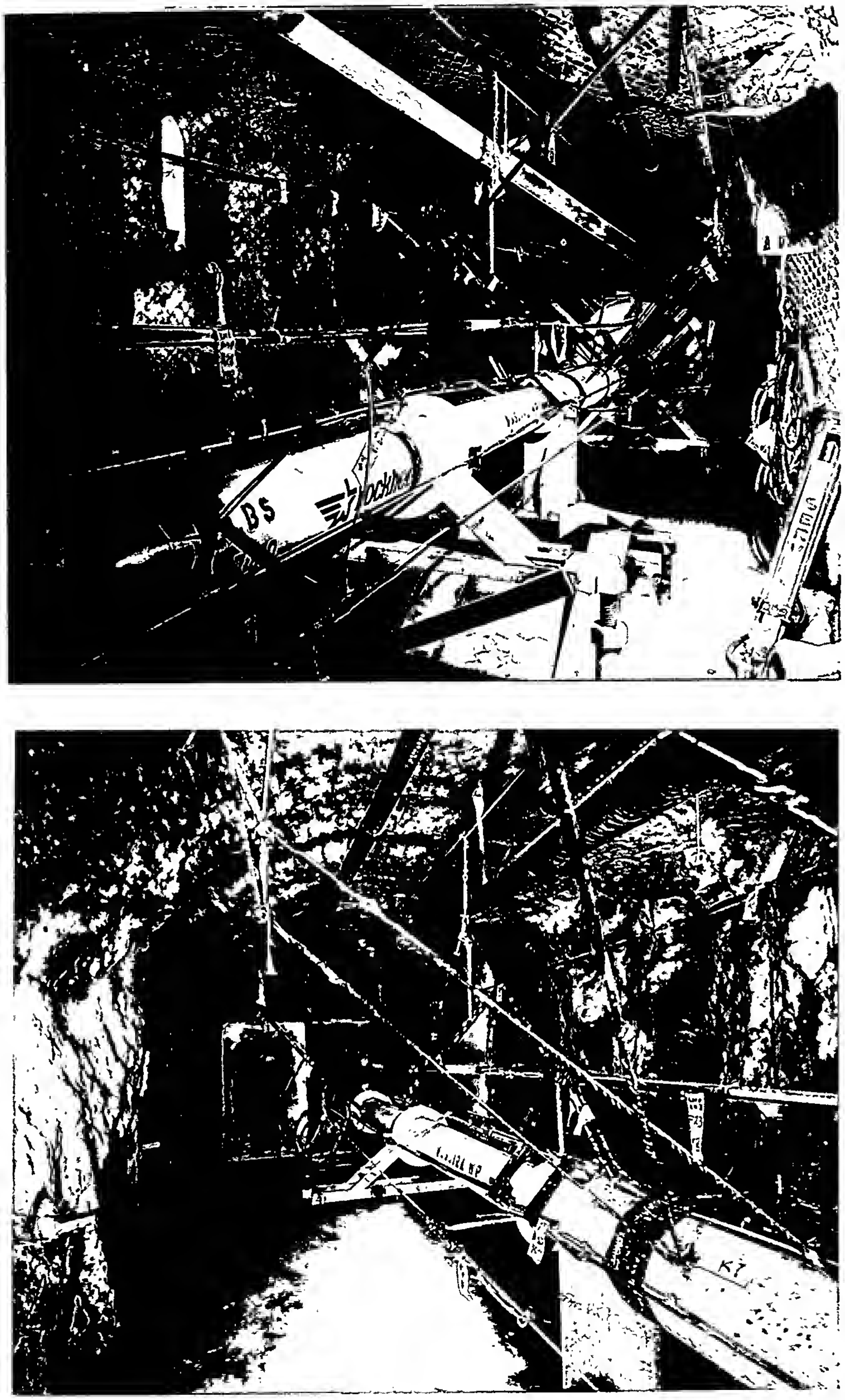

Figure 1. View of the CORRTEX and SLIFER array installed on MIDDLE NOTE taken from near Box-A.

have not yet had an opportunity to examine the MIDDLE NOTE results in detail. Some interesting conclusions can be drawn, but many questions remain. Specifically:

1. The experiments provided an interesting body of close-in diagnostic information, but none of it has yet been carefully compared with calculated predictions.

2. Ground shock propagation in the free field was measured, and it confirmed expectations.

3. The average shock propagation along the LOS as measured by both systems was consistent with previous experience.

4. Cable loops in the CORRTEX and SLIFER systems near the LOS did not perform as they do in the free field. This may be related to partial cable crush by a relatively weak precursor shock.

One item needs special emphasis. This experiment used CORRTEX and SLIFER systems to diagnose close-in effects. Therefore, cables were placed quite close to Box-A and the reverse cone. When these systems are used to measure explosive yield for diagnostic or verification purposes, the cables are arrayed quite differently. Therefore, one should not draw any conclusions from these results concerning the ability of these systems to perform in the verification mode. For good technical reasons the author draws no such conclusion and he urges readers to refrain from doing so.

The following sections will describe the experimental configuration used on MIDDLE NOTE and MISSION CYBER. These were quite similar because the MIDDLE NOTE data analysis was not completed in time to impact the second event except in a few details. Next, results will be presented starting with the straightforward, non-controversial aspects and moving toward the more surprising. The report will conclude with a discussion of the results and recommendations for future study.

It is important to acknowledge that this was a team effort which included important contributions from several agencies. DNA provided overall guidance including field and financial support under Joe LaComb. LANL fielded and recorded the CORRTEX systems under the direction of Don Eilers and Tom McKown. Sandia fielded and recorded the SLIFER systems under the direction of Bob Bass. The first author contributed only the original experimental design and some preliminary data analysis.

2.0 EXPERIMENTAL DESIGN

2.1 DESIGN CONSIDERATIONS.

It has long been recognized that the design procedures and, in particular, the numerical codes used in the process of early time closure design have not been adequately validated by experimental measurements. A number of attempts had been made with limited success primarily because the close-in environment is a most difficult one in which to work.

The CORRTEX measurements made by LANL on TOMME/MIDNIGHT ZEPHER near the working point suggested that the shock wave propagation from the reverse cone was quite different from that predicted by the front end design calculations, [*Ref.* 1]. It was natural then to investigate the possibility of using CORRTEX techniques to map the shock field near Box-A and the reverse cone as an integral test of the design calculations. We recognized that such a measurement would not directly test the calculational assumptions and, the Box-A environment, but it would provide an overall test of the shock field which, after all, was an important result of the design decisions.

During conversations with LANL representatives we learned that they believed the CORRTEX system could be used successfully in the immediate vicinity of Box-A, and also that they were interested in cooperating in such an experimental effort. We also learned that Sandia believed the SLIFER system would provide valid data in the vicinity of the WP.

This led to an elaborate experimental design with several objectives:

1. Use cable systems to measure shock propagation perpendicular to the cylindrical surface and the end wall of Box-A at several axial locations and at several angular orientations. The results would show the evolution of the internal environment and demonstrate the symmetry of that environment.

2. Similarly, measure shock propagation from the reverse cone using cables parallel to the cone and at various angles to it in order to maximize the chances of resolving the amplitude and direction of wave propagation. The results would indicate plasma velocity in the cone as well as the internal cone pressure.

3. Measure shock propagation along the stemmed LOS tunnel at several distances from the LOS pipe. Include cable sections running perpendicular to the LOS to measure directly radial shock propagation. Such data would be directly comparable to a large body of prior SLIFER measurements in the same domain, it would provide information to compare with fluid-coupled plate pressure measurements made in the

same region, and it might give an independent measure of LOS plasma pressure.

4. Cables would also be emplaced to measure the free field ground shock as a yield indication. This experiment was attempted on the MIDDLE NOTE and on the MISSION CYBER events. Since the MISSION CYBER tunnel was made quite small near the reverse cone, the CORRTEX and SLIFER installations were restricted. Aside from this, the cable configurations were quite similar in both cases because there was only a limited opportunity to modify the design for the second event on the basis of lessons learned from the first. The changes that were made will be mentioned later. One change from MIDDLE NOTE was the installation of several different cable types in an effort to clarify some of the surprising results from MIDDLE NOTE and to help further reduce MIDDLE NOTE data.

2.2 BOX-A REGION.

The installation was complex. The following descriptions are inadequate efforts to give the reader at least a feeling for what was done. Reference to Figure 1 may help. On MIDDLE NOTE 14 CORRTEX cables were installed. One failed after installation. There were also 5 SLIFER systems. Six CORRTEX were mounted rnormal to the cylindrical surface of Box-A at distances of 205, 132, and 35 cm from the end wall of Box-A measured along the LOS axis toward the WP. Three were +45 degrees from the vertical, and three were at -45 degrees. These cables ran radially for at least 140 cm. Also, a CORRTEX and a SLIFER ran in the vertical plane upward from the portal corner of Box-A at an angle of 45 degrees. The SLIFER started 10 cm from the corner; the CORRTEX started 20 cm from the corner. In addition, six systems ran from near the end wall parallel to the reverse cone either on it or 30 cm off. Four were CORRTEX and two were SLIFERS.

The MISSION CYBER CORRTEX array also included 14 cables. Seven started about 20 cm off Box-A, ran perpendicular to the cylindrical surface, and then turned parallel to the axis between 60 and 160 cm from the surface. The distances back from the end wall were 1, 59, 134, and 353 cm. (The cable at the end wall ran back toward and around the WP.) Another cable started at the WP and ran parallel to and 90 cm from the cylindrical surface of Box-A. An additional cable started 30 cm from the end wall corner and ran upward at a 45 degree angle. Two cables starting 20 cm from Box-A were taped to the cone. A similar cable ran 27 cm from the cone. Additional diagnostics may have been provided by close-in SLIFER systems[2]. We expected the dense array of diagnostic cables to provide redundant, detailed measurements of shock propagation from Box-A. The results would provide an interesting and perhaps definitive test of an important aspect of early time design.

[2] The author did not have an opportunity to study the MISSION CYBER installation, and he has very limited information about the SLIFER system installed. He has seen no detailed SLIFER results.

2.3 REVERSE CONE REGION.

Since the shock field around the reverse cone was expected to develop later than that around Box-A, the diagnostic cables that had covered the Box-A vicinity could also be arrayed around the reverse cone without risk to early data if elementary precautions were observed. The basic diagnostic plan was to measure shock propagation from the cone with CORRTEX and SLIFER cables installed parallel to and at various angles to the cone.

The paragraphs above mention the location of the parallel cables close to the cone. On MIDDLE NOTE five other CORRTEX cables zig-zagged toward and away from the cone. They looked something like large W's with their bases about 1 to 2 cm from the exterior cone surface. One SLIFER also looped away from and back to the cone surface, but since it was relatively stiff compared to the CORRTEX cables, its bends were quite gradual. In addition, a SLIFER cable was stretched 100 cm from the cone and parallel to it.

The MISSION CYBER CORRTEX array included five cables that lay parallel to the cone at separations between 0 and 100 cm. There were five other cables that zig-zagged back and forth. This time the closest approach of the cable bends was about 5 cm from the cone.

2.4 LOS TUNNEL.

The cables that had been arrayed to diagnose the Box-A and reverse cone regions continued along the tunnel to study shock propagation in the stemming region. Cables were run in contact with the pipe flanges which made them 8 to 10 cm from the pipe wall as well as at separations of 30 and 60 cm. These configurations were much like the SLIFER arrays conventionally used on DNA events. Other cables ran along the tunnel walls. In order to avoid any possible complication arising from cable proximity to the WP, several cables started at the pipe flange at the end of the cone. A prominent feature of the CORRTEX installations was the inclusion of dynamic calibration loops at many places. Prior testing experience had shown that doubling the flexible cable back on itself would introduce a predetermined discontinuity into the record at a precisely known location thereby calibrating the system at the time of use. These loops were customarily 1 m long thereby introducing 2 m, easily identified discontinuities into the records. In the field these loops were made by folding a 1 m-long length of cable back on itself and taping the three strands together with plastic tape. The strands were in intimate contact, and the diameter of the cable bend at both ends of the loop was about 4 cm.

Additional shock front detail was sought by installing cable loops in several places that ran at an angle to the main cable. On MIDDLE NOTE these loops were all "down loops" in that they ran from the cable path parallel to the LOS pipe, "down" toward the LOS pipe. On MISSION CYBER these were augmented by "out loops" and by loops installed at a 45 degree angle from the cable pointing away from the WP.

One surprising observation on MIDDLE NOTE was the apparent failure of some of the cable loops to respond as expected. Therefore, on MISSION CYBER one cable (K4) was run 89 cm from the LOS without loops of any kind. It was adjacent to two other cables (K6, K8) containing standard loops. K4 and K6 were the same type of cable.

Figure 1 shows two photographs of the MIDDLE NOTE configuration as viewed from near Box-A. These words and figures could not possibly describe the cable installation adequately. Nevertheless, it is hoped the reader gets the impression of an elaborate, interlocking array of diagnostic cables that could provide detailed information about the shock configuration as a function of time around the several regions of an LOS installation.

2.5 FREE FIELD REGION.

Two cables on each event were run along the bypass drift in an effort to measure free field shock propagation. On MISSION CYBER a third cable was emplaced in a drill hole parallel to the LOS drift. The early parts of the cables were used to diagnose the Box-A region and, in some cases, to study the environment near add-on experiments in alcoves along the bypass drift.

2.6 ADDITIONAL DETAILS.

Additional construction details should be mentioned. The CORRTEX and SLIFER cables were supported on messenger cables tightly stretched between solid anchors. The cables did not sag noticeably. Two things happened automatically because of the number of cables used. Cables crossed at well defined places. This means that any difference in reporting time at a crossing point may be an indication of system response time or of some diagnostic difficulty. The other is that on several occasions two or more cables were supported on the same messenger. They were taped together for an extended distance. Such cables were expected to report simultaneously. An important question involved the absolute location of the various points on the cables relative to the WP and other features. Individual cables were given length marks before installation. For instance, each CORRTEX cable had a number taped to it every meter that indicated the cable length starting from zero at its WP termination. These numbers were often used to identify installation features such as cable loops. As an example, a 16-14 loop would have the 14 and 16 m tags placed in immediate proximity and the resulting cable loop dressed on the cable away from the WP.

Several measurements were made of the locations of various features of the installation relative to, say, Box-A or the LOS pipe. Cable lengths where cables crossed were also noted. In addition, conventional surveys were made at hundreds of points to provide an independent measure of cable location. This survey data was essential to the reduction of the free field records which could obviously not be related to otherwise known LOS features.

Finally, mention should be made of the fact that the CORRTEX recorder used was the newly developed and tested Model III unit. The CORRTEX system was augmented by several digitizers that recorded analog transmitted and reflected pulses. Digitizer rates of 20 and 100 MHz were used. The analog record was intended to provide data on the effect of EMP on the data pulses. The SLIFER design was similar to that used successfully for over 25 years.

Several types of cable were used in the two installations. Table 1 identifies a specific diagnostic cable with a particular cable type, and Table 2 briefly characterizes the various cable types. The "crush strength" listed is the approximate value quoted by LANL. It is not based on reproducible measurements.

Table 1. The type of cable used for each system on each event.

MIDDLE NOTE		MISSION CYBER	
System	Cable Type	System	Cable Type
K1	FSJ1-50	K1	FSJ1-50
K2	FSJ1-50	K2	FSJ1-50
K3	Lost	K3	FSJ1-50
K4	FSJ1-50	K4	FSJ1-50
K5	FSJ1-50	K5	FSJ1-50
K6	FSJ1-50	K6	FSJ1-50
K7	FSJ1-50	K7	RF-58
K8	FSJ1-50	K8	RF-174
K9	FSJ1-50	K9	RF-174
K10	FSJ1-50	K10	RF-214
K11	FSJ1-50	K11	FSJ1-50
K12	FSJ1-50	K12	FSJ1-50
K13	FSJ1-50	K13	RF-58
K14	FSJ1-50	K14	RF-174
SL1	HJ4-50	SL1	HJ4-50
SL2	HJ4-50	SL2	HJ4-50
SL3	HJ4-50	SL3	HJ4-50
SL4	HJ4-50	SL4	FSJ1-50
SL5	HJ4-50	SL5	HJ4-50
		SL6	HJ4-50
		SL7	HJ4-50

Table 2. Characteristics of cable types used in the MIDDLE NOTE and MISSION CYBER CORRTEX and SLIFER experiments.

Type	Dielectric	"Crush Strength"
FSJ1-50	Foam	~1 kb
RF-58	Foam	~0.1-0.6 kb
RF-174	Foam	~0.1-0.6 kb
RF-214	Solid	25-30 kb
HJ4-50	Air	1.0 kb

3.0 RESULTS

The following sections will present some of the results obtained from CORRTEX and SLIFER systems installed on the MIDDLE NOTE and MISSION CYBER events. No effort will be made to be exhaustive in this effort simply because the full analysis of MIDDLE NOTE results by LANL has not yet begun and the MISSION CYBER work is not completed. Nevertheless, much of the data available leads to redundant conclusions. The order of presentation will be just opposite of that used in the last section simply because this organization presents the most easily interpreted data first.

3.1 FREE FIELD RESULTS.

CORRTEX cables were used on both events to measure the free field ground shock insofar as that can be done in the bypass drift. This assumes mining and grouting processes did not alter the results. Figure 2 shows the beautiful record of cable shortening as a function of time for MISSION CYBER[3]. Note the crisp discontinuities at a number of locations along the record. In general, these correspond well with the cable loops as listed in Table 3. All data discussed below is from MISSION CYBER unless otherwise indicated.

Clearly a smooth trajectory will be obtained when the calibration steps are removed. Survey data is available which locates the cable run in considerable detail. It will be possible to use this information to provide excellent shock propagation information and a yield estimate.

[3] Some of the figures to follow have an ordinate of "crush length". This is actually also correct because CORRTEX crush length is measured along the cable (including loops)from the WP end of the cable. Crush length and cable shortening are numerically identical. Other figures have an ordinate of "cable length". This is measured along the cable path from the WP end of the cable with known losses such as cable loops removed.

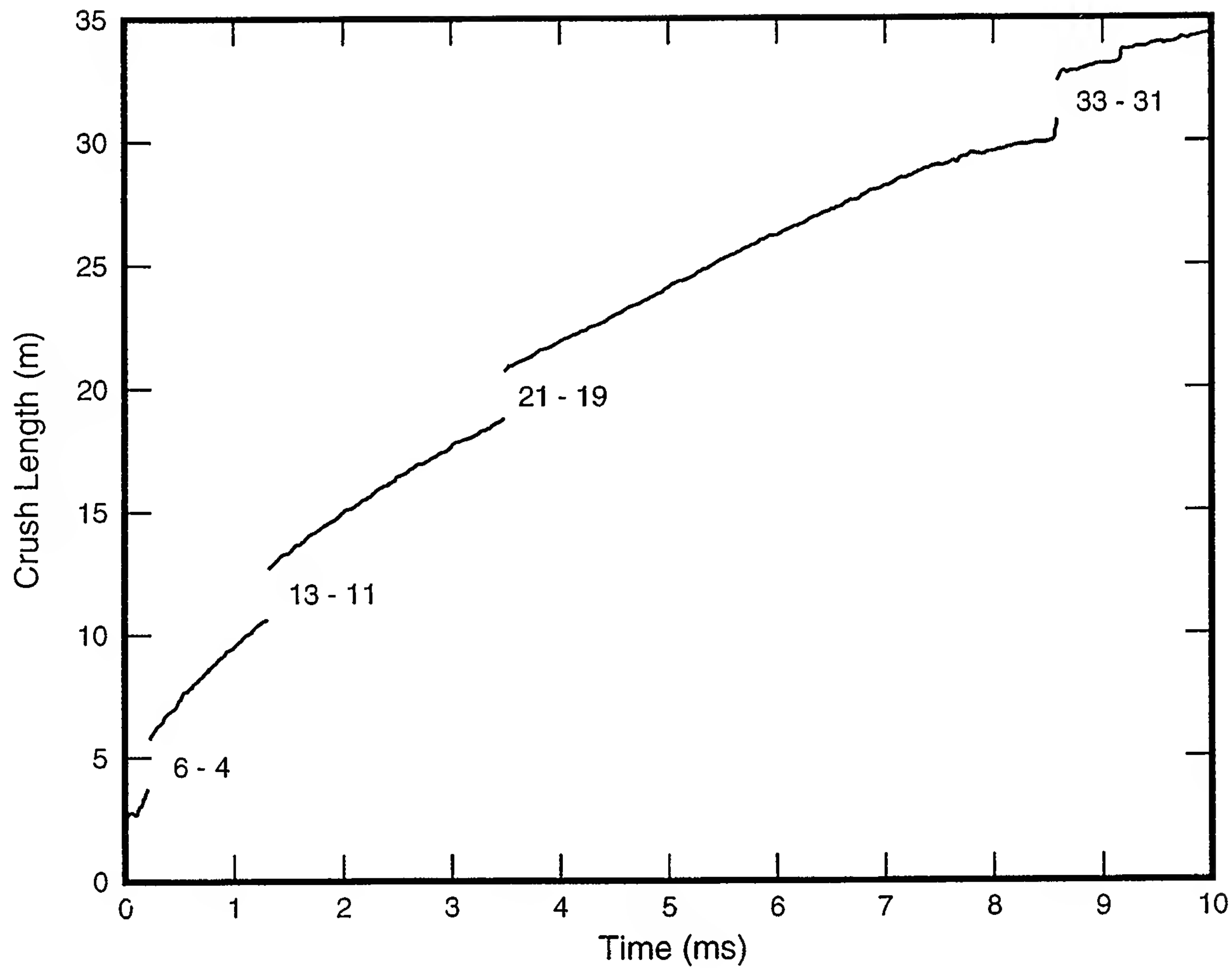

Figure 2. Raw data showing cable crush of K1 as a function of time along the bypass drift on MISSION CYBER.

Table 3. Cable calibration loops installed in CORRTEX cable K1 installed on MISSION CYBER.

Loop	Location
1	6-4
2	13-11
3	21-19
4	33-31

The early part of cable K2 was laid out in a rectangular grid with segments parallel to the LOS axis. They were crossed by K1 at roughly 1 m intervals. Figure 3 is a scaled diagram of the K1, K2 and K3 installation produced from the survey results and referenced to WP. The approximate location of Bos-A and the LOS are also shown. The crush record of shock front-cable interactions shows alternating sharp and rounded steps as the wave engulfed successive grid lines. This behavior is shown in Figure 4 where the positions along K2 (A, B, C, etc. in Figure 3) are shown as crush length levels in the data. When the remainder of the data is reduced, it agrees very well with that from K1.

As shown in Figure 3, cables K1 and K2 cross at four places (labeled W, X, Y and Z). Table 4. lists the locations and CORRTEX times for these intersections. Note that no times are listed at the second of the stations on K2. This is because these ranges happen to fall in a region where cable response jumps from one end of the cable to the other. The fourth intersection could be determined because the K2 cable doubled back on itself in this interval. The agreement in reporting time of the two cables is good. It is comparable to the time between CORRTEX pulses.

Table 4. Cable lengths and crush times for K1 and K2 cable crossings on MISSION CYBER.

Point	Cable Lengths (m)		Crush Time (ms)	
Label	K1	K2	K1	K2
W	7.147	18.090	0.410	0.414
X	8.260	21.725	0.685	-----
Y	9.373	29.445	0.886	0.895
Z	10.482	39.490 (32.510)	1.180	1.195

In general, the CORRTEX cables installed on MIDDLE NOTE and MISSION CYBER to measure free field ground shock performed as similar cables have routinely performed on other nuclear tests. A few comments on how well ground shock was measured will be presented in Section 4.

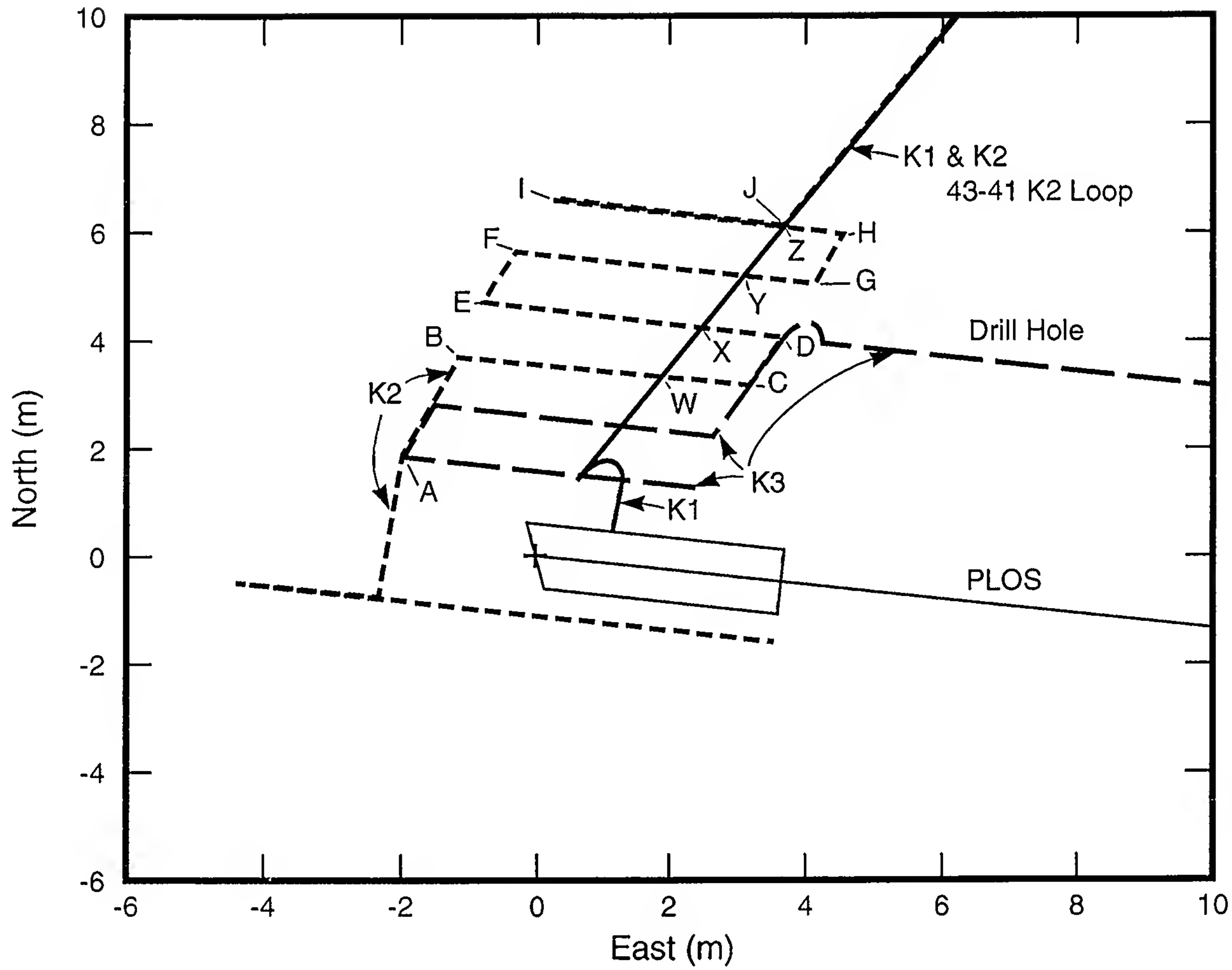

Figure 3. CORRTEX K1, K2, and K3 installation on MISSION CYBER.

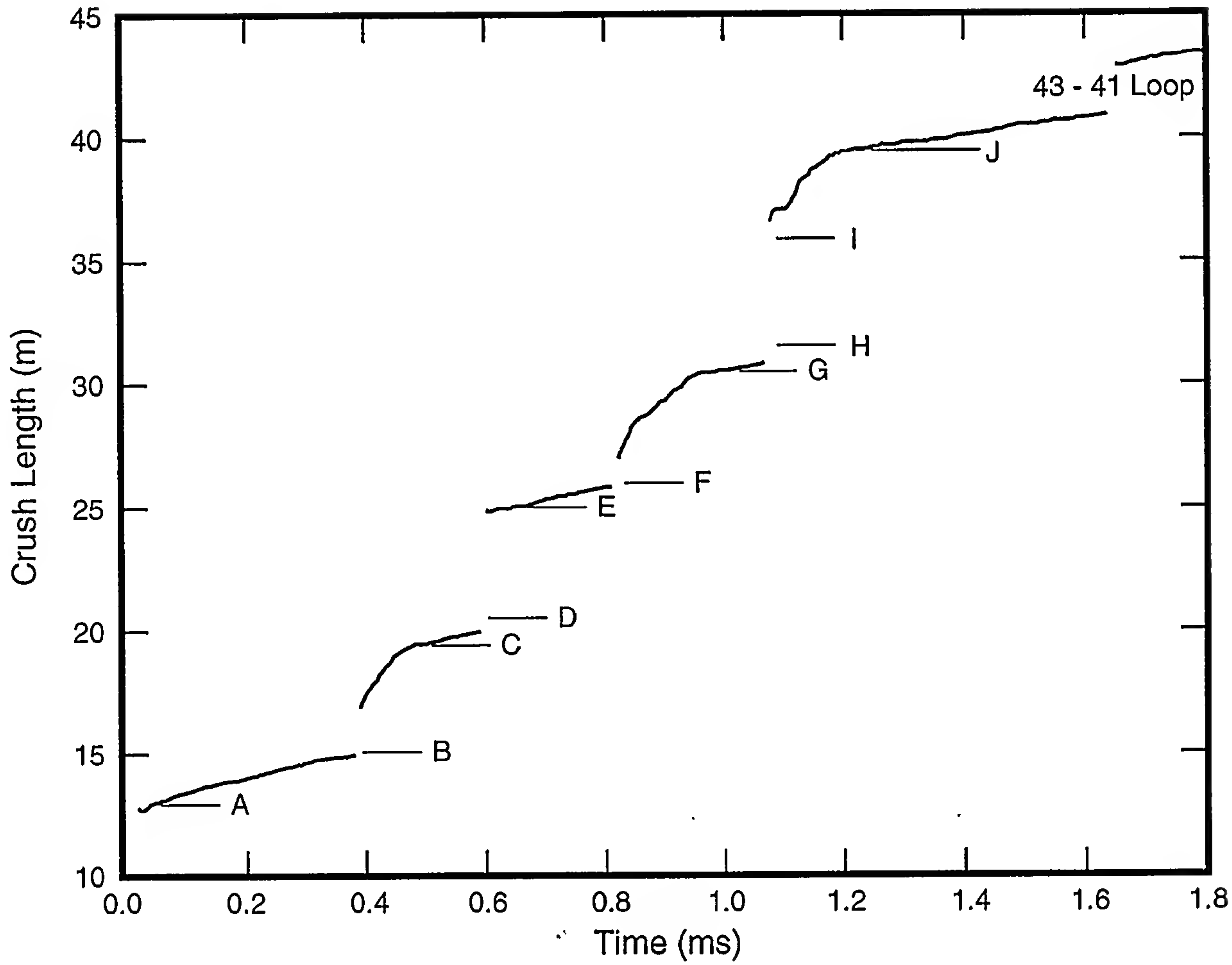

Figure 4. CORRTEX response of cable K2 mounted on the ceiling of the buypass diagnostic alcove on MISSION CYBER.

3.2 STEMMING REGION RESULTS.

Now the data becomes more controversial if looked at in detail. But much of it is quite comparable to results routinely obtained on HLOS events as illustrated by the SLIFER summary for MIDDLE NOTE. This was prepared by Sandia and shown as Figure 5. Note that SL1 mounted on the pipe flanges reports earlier and to a greater range before the knee than do SL2 and SL3 which were mounted 30 cm off the pipe. SL5 near the tunnel wall saw only ground shock. The other SLIFER cables showed the usual Brownlee knee and the customary first and second plasma flows (lines and numbers added). The MISSION CYBER SLIFER data was of comparable quality.

Before proceeding with a discussion of the results, a digression to describe how CORRTEX records are analyzed would be in order. Loops are routinely installed in the cables at precisely known locations. When the shot records are examined, sharp steps like those seen in Figure 2 are usually seen at locations quite close to the known locations of cable loops. The CORRTEX data reduction requires a value for the speed of electromagnetic wave propagation along the cable. The preliminary look at the data uses the nominal value of this propagation factor. A more precise value is determined by effectively changing the factor until the steps in the record appear where they are known to be from measurements along the cable at the time of cable construction and installation. Usually the change required is less than one percent.

The surprising thing about MIDDLE NOTE and MISSION CYBER CORRTEX data is that very few of the cable loops located beyond the vicinity of Box-A reported as expected. Such an observation is not without precedent. LANL has seen similar results from CORRTEX cables exposed to low stress levels below the crush threshold of the cable. That is probably what is involved here.

The situation is illustrated by Figures 6 and 7. The first shows a cartoon of how a part of cable K5 is laid out. First we see a run of cable parallel to the pipe. Then the cable bends toward the LOS and back to its original line. To first approximation the range station values along the cable do not change on the "down loop". Next comes a standard loop in which a two meter length of cable is bent back on itself. Now the range station graph becomes "N" shaped as shown. Beyond the loop the range increases uniformly.

Figure 7 shows the raw data and an elementary interpretation of it. In the interval between about 2.2 and 2.9 ms the CORRTEX report moves down the loop and back up it. In the interval between 3.1 and 4.0 ms the signal indicates the shock goes down the horizontal loop, turns around and goes back toward the WP, and then moves smoothly away. We have found nobody that believes the shock from a nuclear detonation does any such thing. The question is, What information can be extracted from these results? In the following paragraphs and sections an analysis

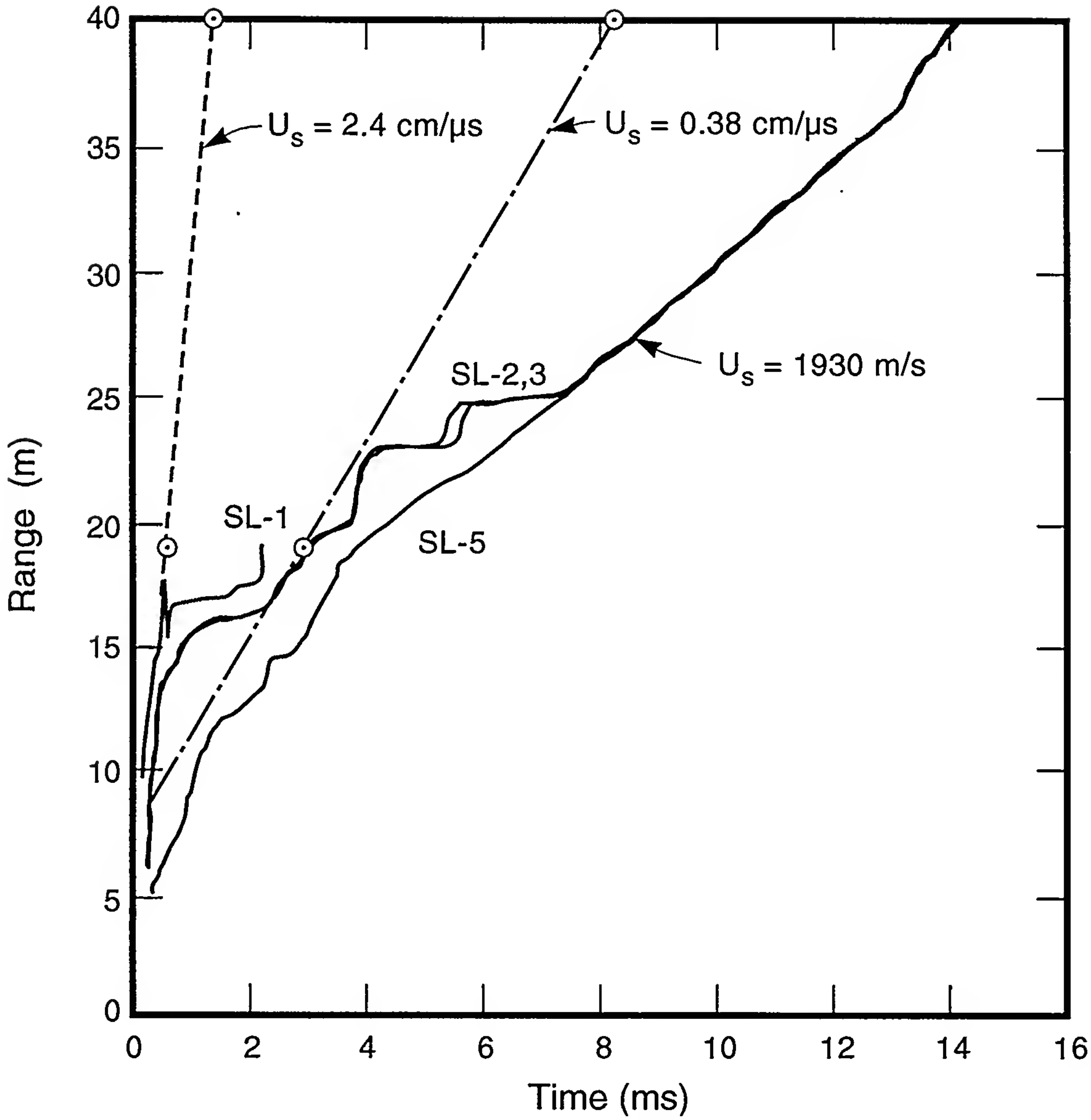

Figure 5. SLIFER data from MIDDLE NOTE.

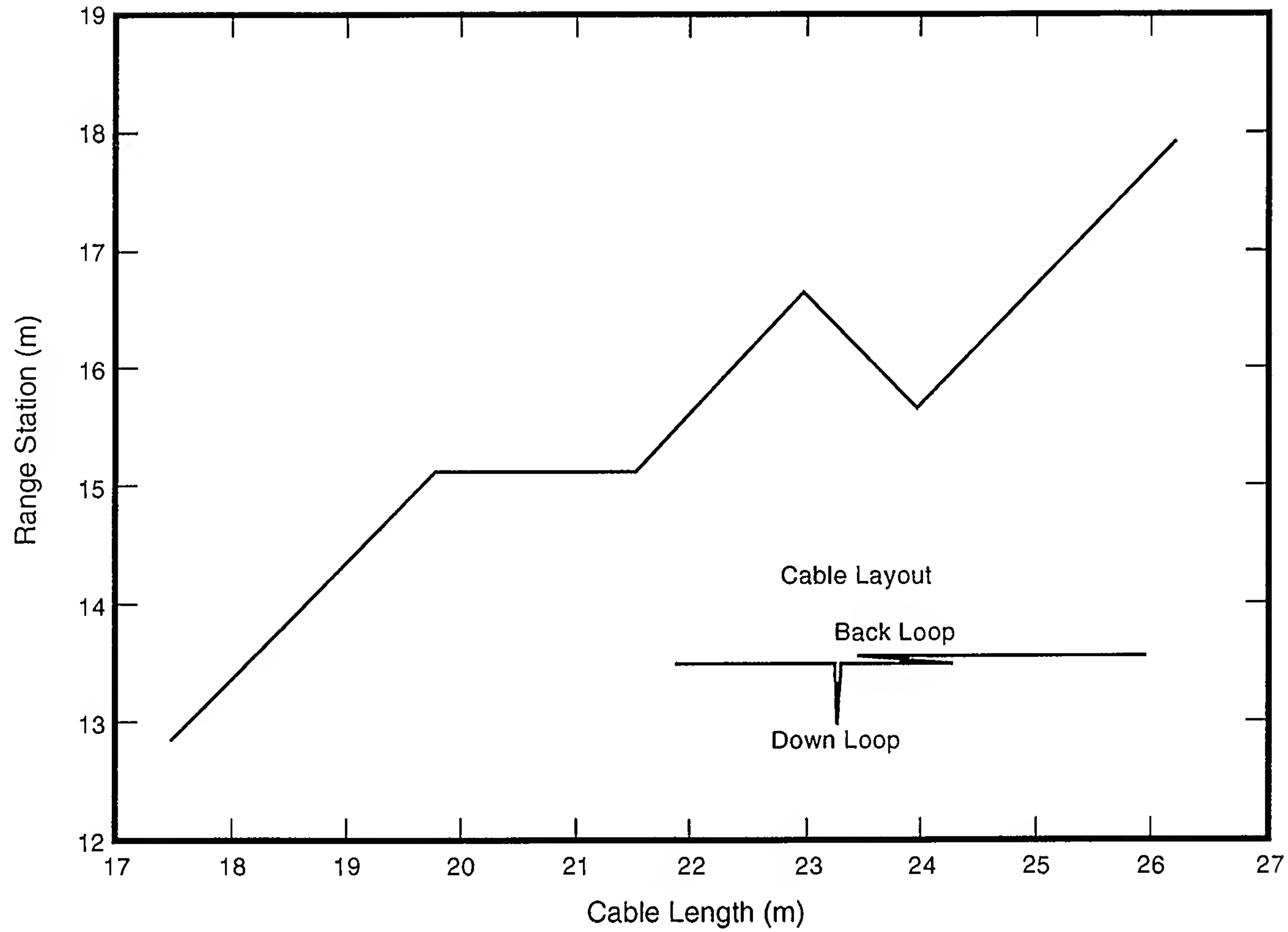

Figure 6. Relation between cable length and range station for MIDDLE NOTE CORRTEX cable K5.

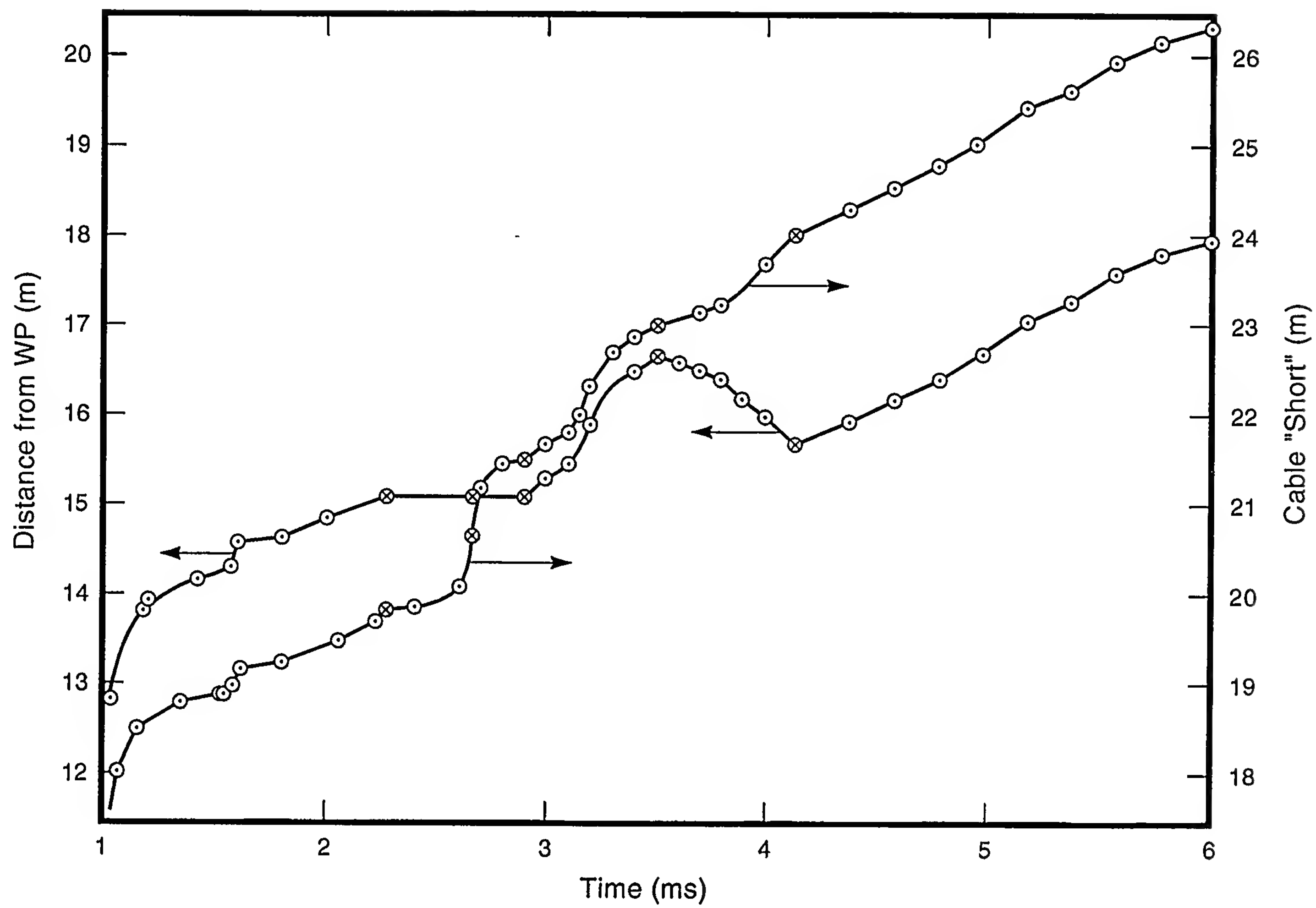

Figure 7. Raw data and distance-corrected CORRTEX data from cable K5 on MIDDLE NOTE.

will be described and evaluated followed by speculations as to the cause of the CORRTEX behavior.

LANL has made a "routine" analysis of the data as illustrated in Figure 8 which shows data for K6. The known locations of loops were identified on the raw data trace. They fell in or near regions of rapid change. The cable propagation factor was adjusted to center the loops as well as possible in regions of rapid apparent cable crush. The result is shown as "Adjusted data", and the six loop locations are indicated. Finally, the known lengths of the loops were subtracted from the overall cable length giving rise to the curve marked "Final Results". Note that data which fell in a region which should have been clipped was simply discarded. This caused the gaps in the trace. This process is shown in more detail in Figure 9 which considers only data near the 19-17 loop in K6. Some plot programs connect points at the edges of such gaps even if there is no data. This artifact makes some of the traces shown appear better than they are.

Cables K4 and K6 were both type FSJ1-50, and they ran together about 89 cm from the MISSION CYBER LOS. K6 contained three regular loops and two down loops while K4 contained none. It was included after MIDDLE NOTE showed a problem with loops to provide a simple reference data trace. Figure 10 shows the final data from the two cables. (Since K4 had no loops to provide a basis for calibration, the propagation factor for K4 was adjusted so K4 and K6 agreed over an extended internal beyond the loop region.) The overall agreement between these records is excellent. It provides reassurance that the peculiar behavior observed in loop regions does not contaminate the basic data.

Note two points however. The K6 record runs ahead of K4 as each of the three loops is approached. It appears as if the cable report sees a loop coming and speeds up to meet it. Also note the signal jump at 4.1 ms in K6. This is a brief indication of an open rather than a shorted cable. These two points will be discussed in more detail later.

A comparison of the K6 record and that from K1 which approximates a free field trace is shown in Figure 11. Clearly a signal runs along the LOS ahead of ground shock, as we know. The pipe signal essentially dies out within 10 ms.

Figure 12 shows a comparison of five records all running about 90 cm from the LOS. K4, K6, and K8 were bundled together on the miners-left side of the tunnel, and K5 and K7 were together on the miners-right side. The overall agreement is quite good, and the general features look much like SLIFER records. The influence of the first and second flows can be identified.

Records from four cables run parallel to the LOS are shown in Figure 13. The ranges are along the flanges, 1, 2, and 3 ft from the pipe. All the cables except K9 which was mounted on the flanges were type FSJ1-50. K9 was RF-174 which was thought to be particularly weak.

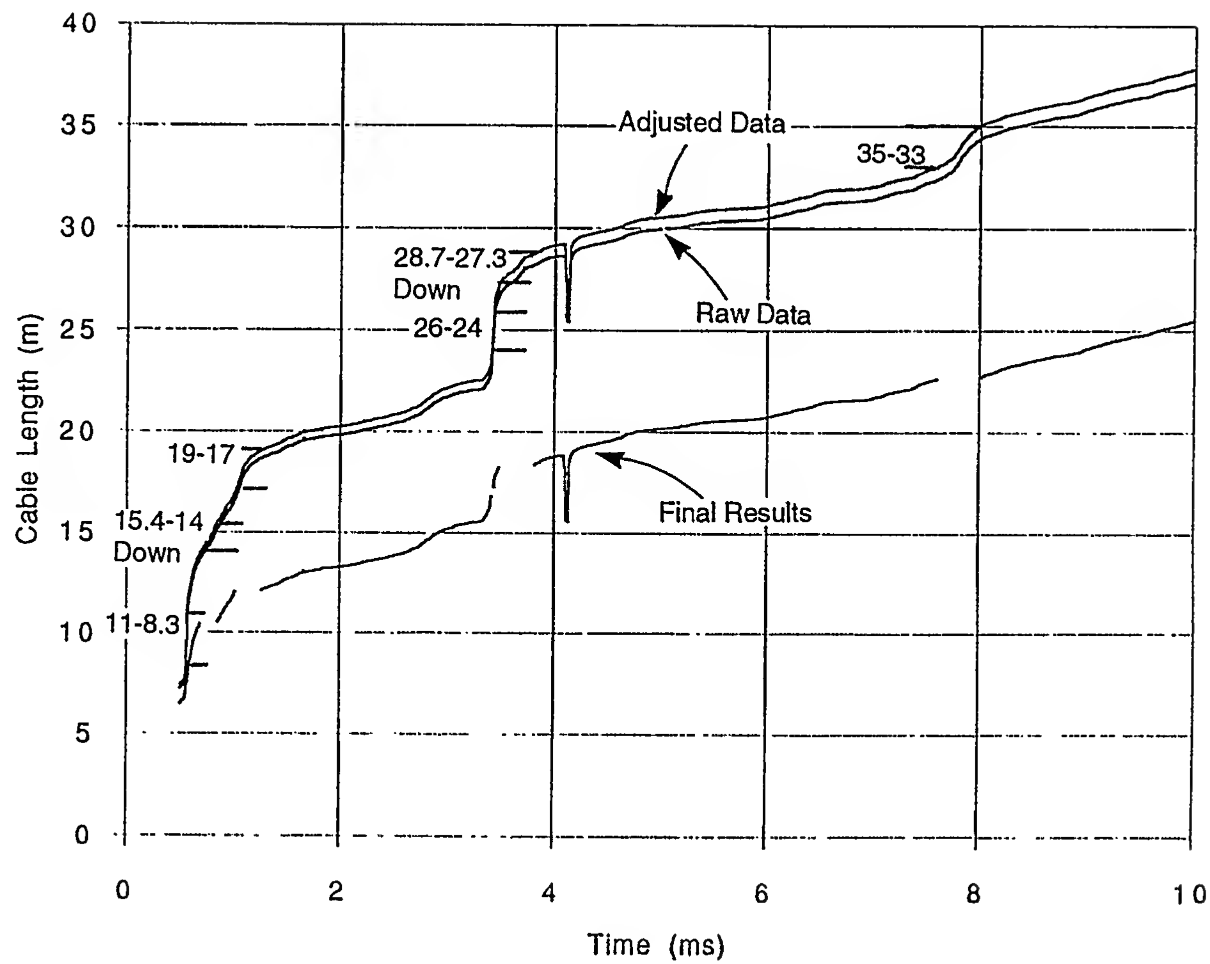

Figure 8. Illustration of the data analysis procedure used for CORRTEX records.

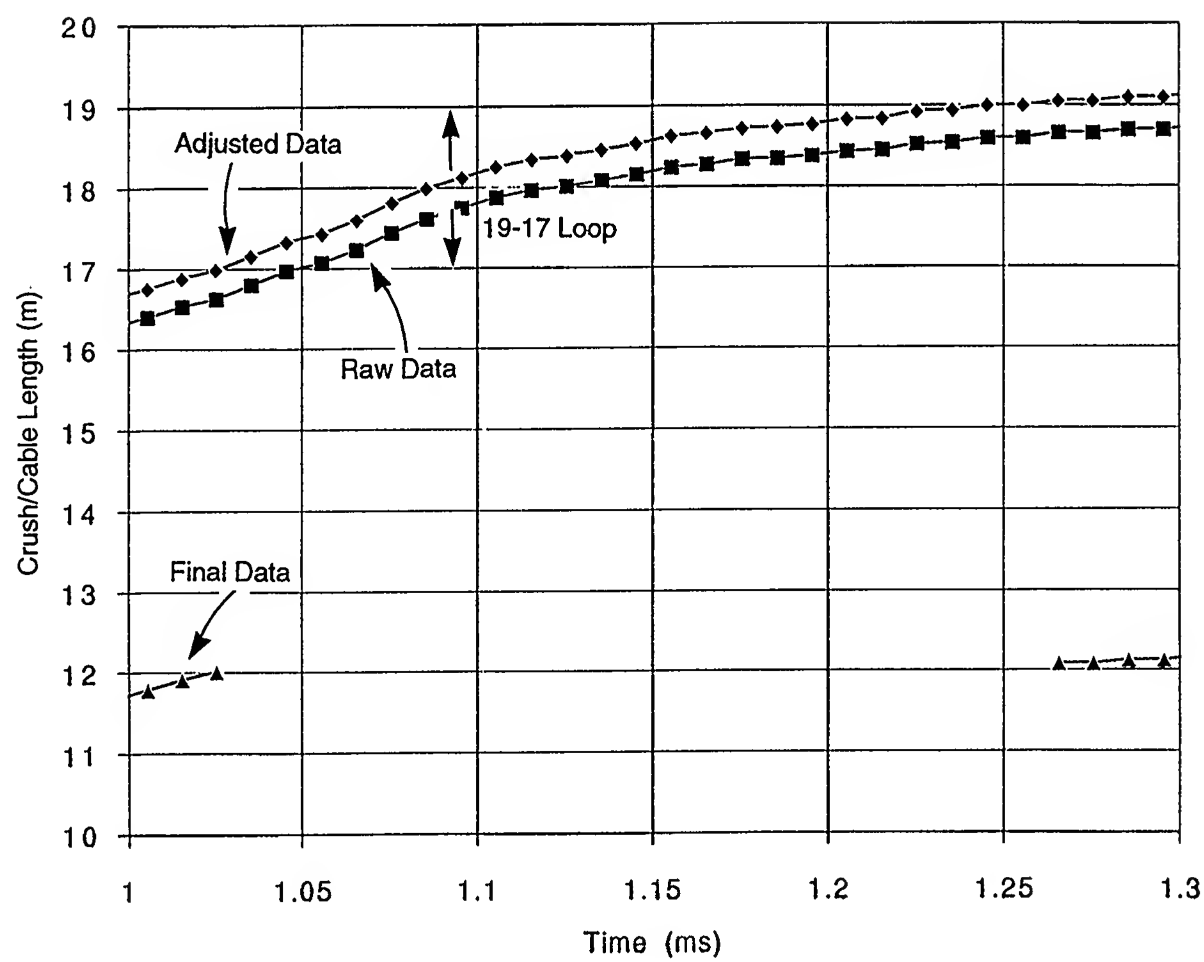

Figure 9. Detail of the CORRTEX cable calibration process.

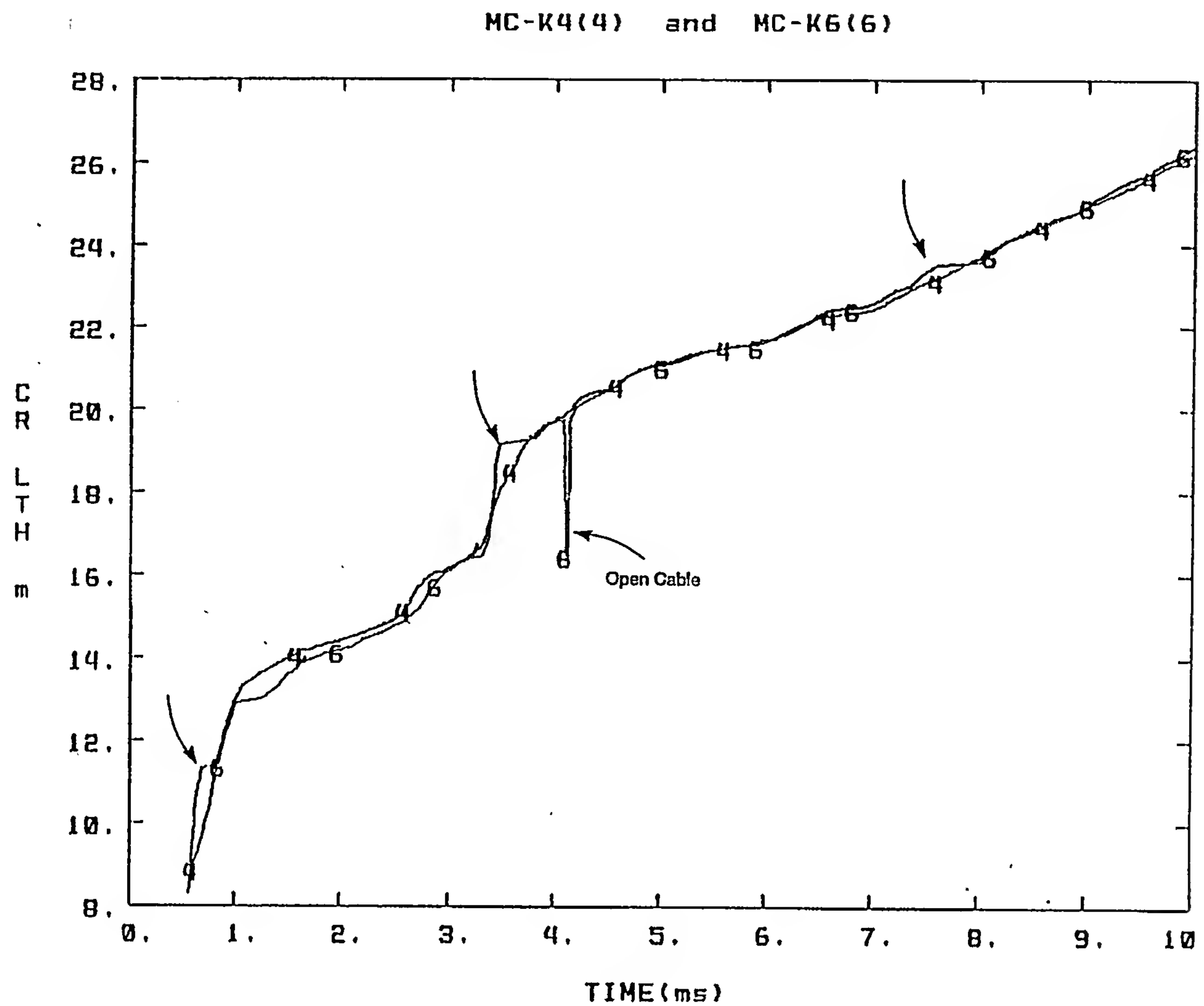

Figure 10. Comparison of cable length measurements on MISSION CYBER K4 and K6 after cable loops were removed.

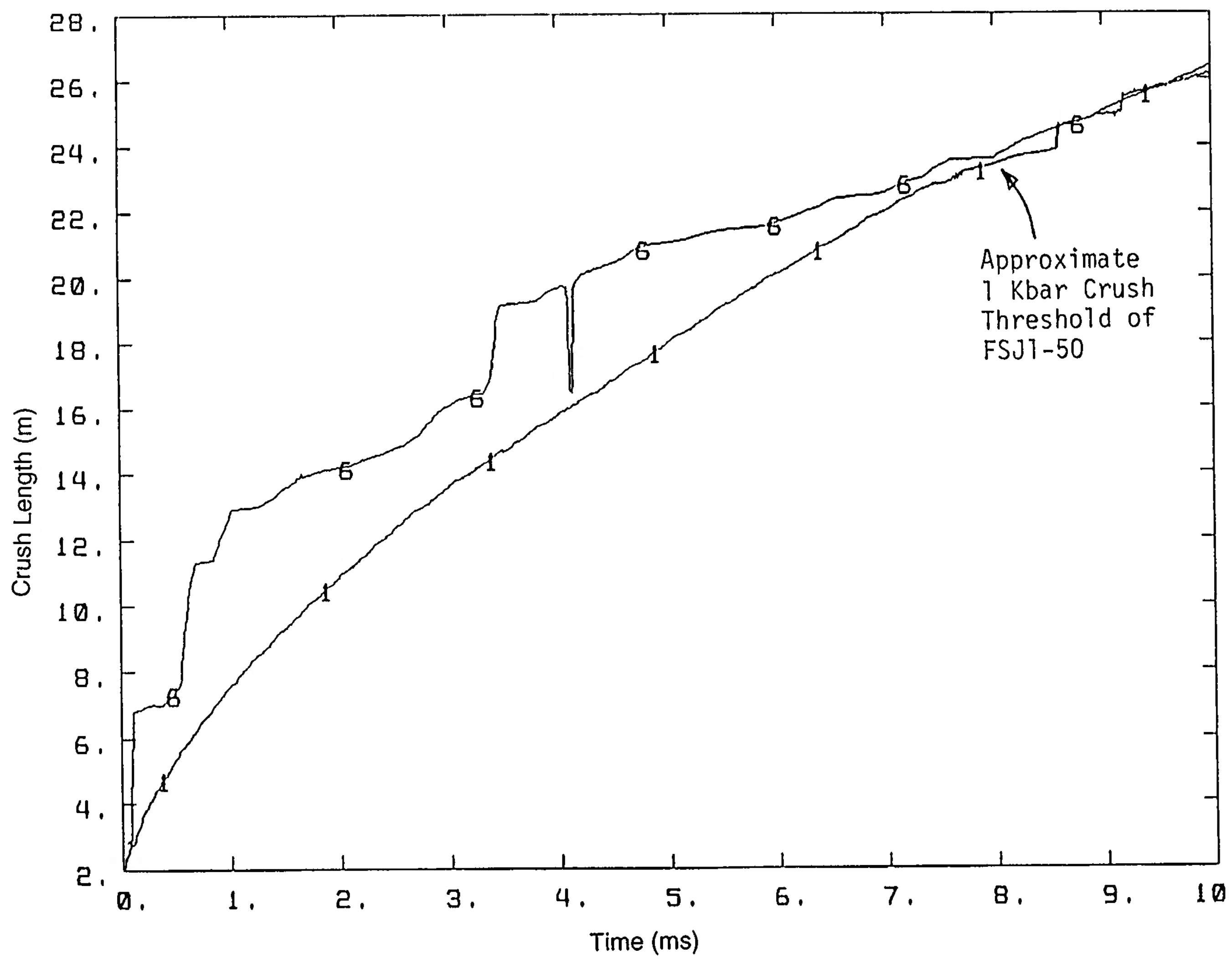

Figure 11. Comparison of reduced data from MISSION CYBER K1 and K6 showing evidence for a fast moving signal in the LOS pipe.

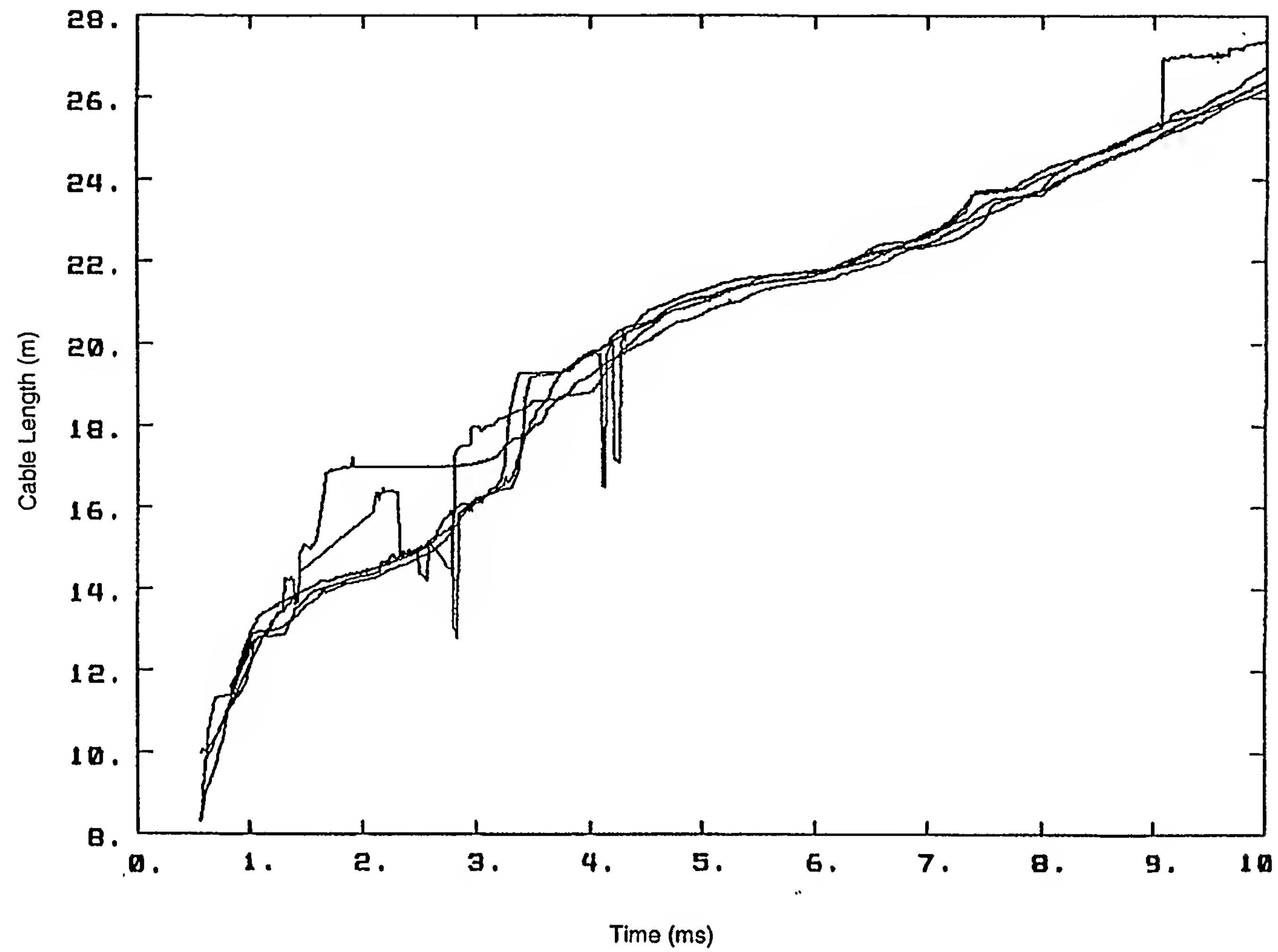

Figure 12. Comparison of reduced CORRTEX records from five separate cables run in two groups down the LOS tunnel of MISSION CYBER. Three of the cables were FSJI-50, one was RF-58, and the last was RF-174.

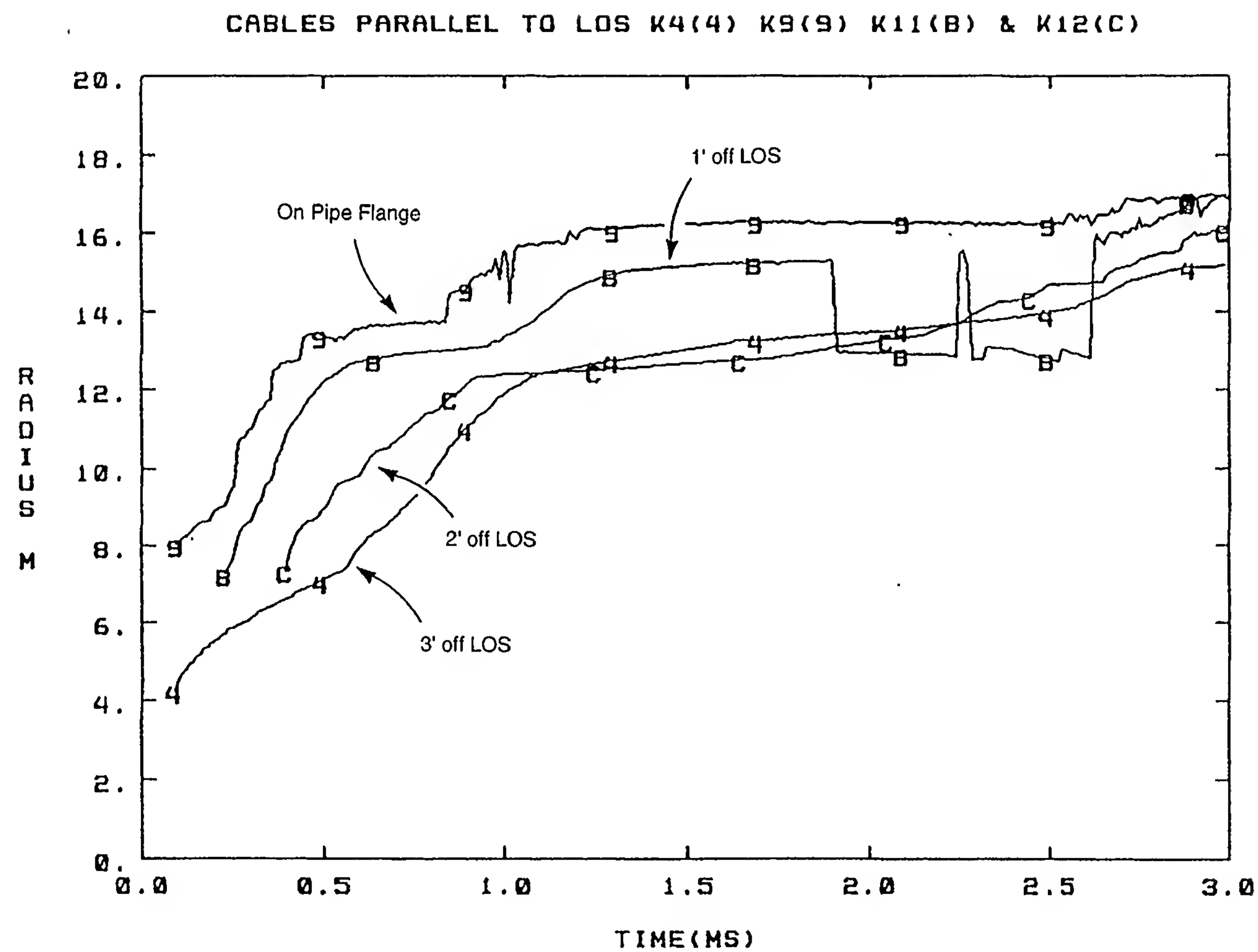

Figure 13. Comparison of MISSION CYBER CORRTEX records from four cables run at distances of 8 cm, 1 ft, 2 ft, and 3 ft from the LOS.

The general trends of these records are comparable to the familiar SLIFER records, but the details are not presently understood.

Another comparison may be instructive, but it is emphasized that this one involves raw data. Figure 14 shows the pressure from a fluid coupled plate gage mounted at a range of 18.9 m in the LOS wall for the MIDDLE NOTE event. This range is near the limit at which the CORRTEX system is expected to operate. CORRTEX cables K9, K10, and K11 ran along the pipe flanges in this interval. The uncorrected times of their reports are shown on the figure. The spread between them may well be reduced when the CORRTEX records are analyzed, but it seems clear that the first plasma pulse hit those cables with an amplitude near 0.3 kb (inside the LOS pipe) perhaps as much as 5 ms before they reported. The nominal crush strength of the cables was 1 kb. The interesting question is, what caused the CORRTEX report? It apparently wasn't ground shock which arrived about 3 to 5 ms later.

Many other records are available, but must await the completion of the data analysis by LANL for a complete presentation of the results for both events. The final analysis for MIDDLE NOTE has not yet begun.

3.3 REVERSE CONE RESULTS.

A dense array of cables was installed near the reverse cone to diagnose the interaction between the flow in the pipe and the ground shock. Much apparently good data was obtained, the cable loops worked well, and interesting conclusions can be drawn. The data from several cables stretched along or parallel to the cone are shown in Figure 15. The location of the reverse cone is indicated by horizontal lines near 3.7 and 7.1 meter ranges. There are so many things to note that it's impossible to do them justice here. Consider:

1. The relatively strong, solid dielectric cable RF-214 used as K10 was taped to the reverse cone. Note that it did not show a report beyond the cone until 0.55 ms. It did not see the plasma flow. The record shows a Brownlee knee by 0.4 ms. This cable was just too strong for the MISSION CYBER environment. This, in itself, is useful information.

2. Cable K11 was also taped to the cone, ran about a meter beyond along the LOS, then it looped back toward WP and ran 1 ft off the pipe toward the portal (see diagram in Figure 15). It reported to a range of 6 m immediately, and saw the plasma pulse beyond the cone before 80 μs. This cable also gave signals at two radial distances from the axis beyond the cone.

3. Cable K12 was run 1 ft from the cone. For the first 0.2 ms it reported almost simultaneously with K10 on the cone. Then it saw the pipe blowout just beyond the cone. The cable continued parallel to the LOS

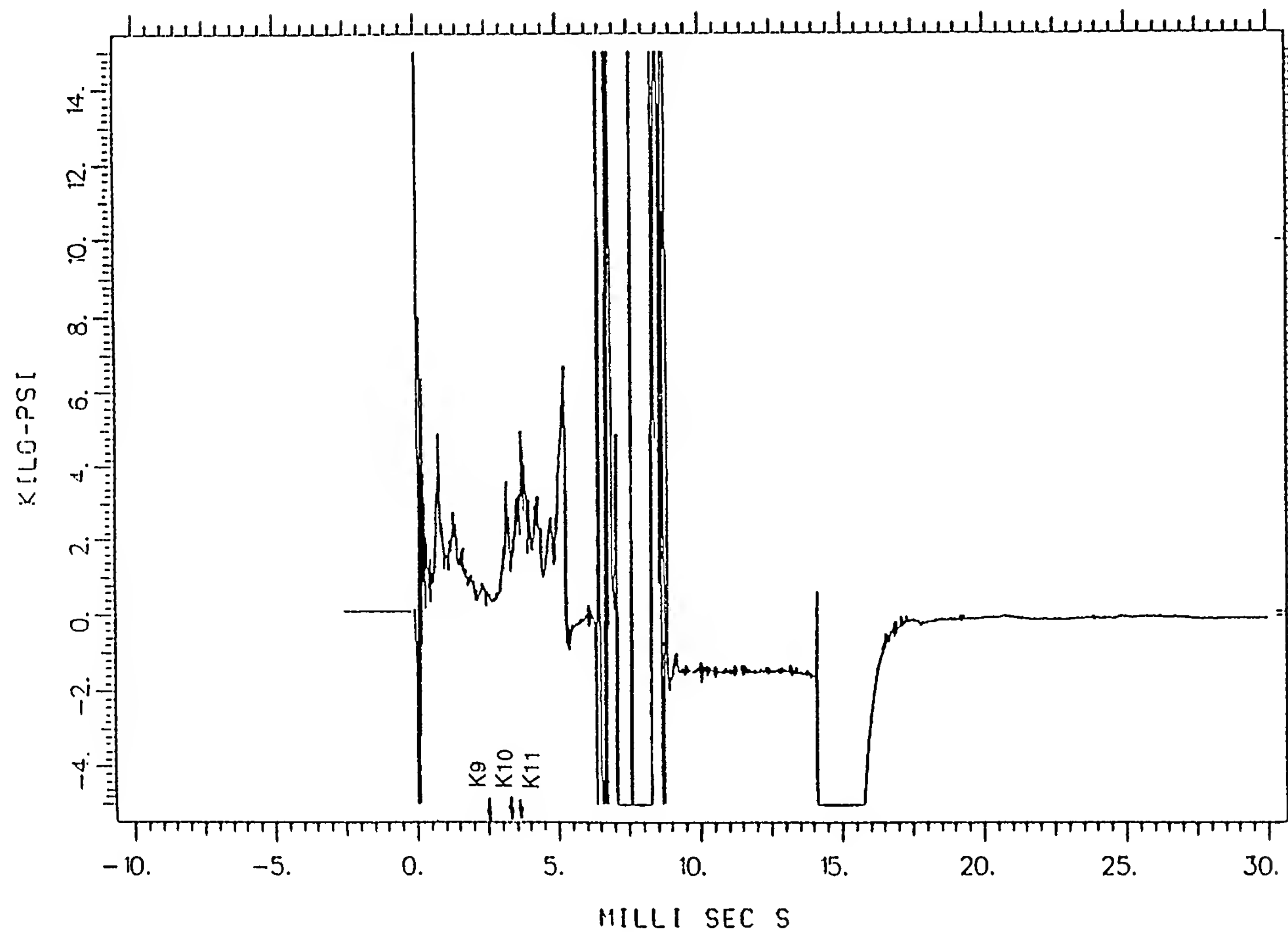

Figure 14. Fluid Coupled Plate record of plasma pressure in the MIDDLE NOTE LOS range of 18.9 m. The uncorrected times of three nearly CORRTEX cable "reports" are indicated.

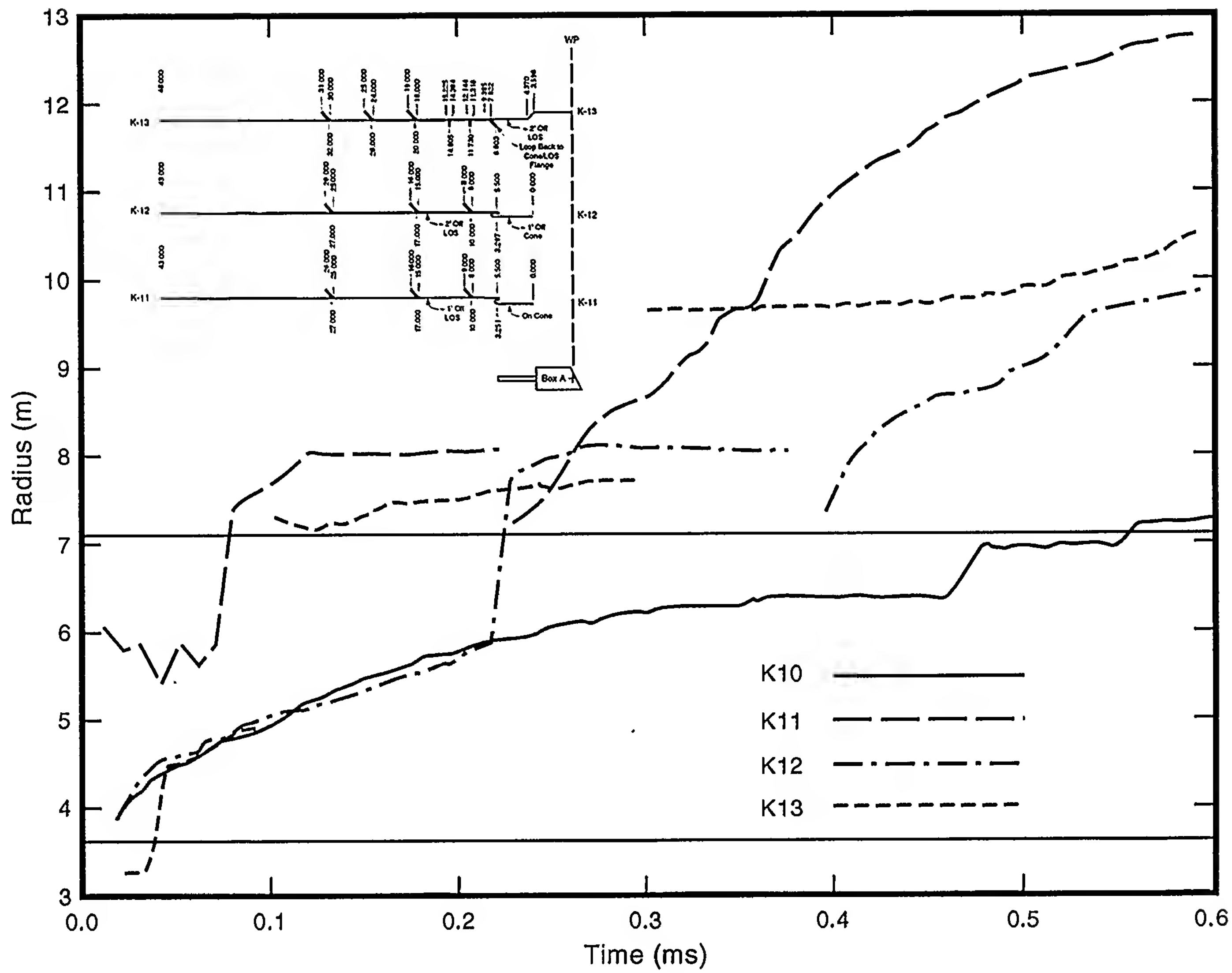

Figure 15. MISSION CYBER CORRTEX data from cables near reverse cone.

surface for about a meter; then it looped back toward the WP and ran 2 ft from the pipe toward the portal. The record shows a signal reflection along both segments of cable.

4. Cable K13 ran 2 ft off the cone and for a short distance beyond, then doubled back to the cone—LOS flange at a 45° angle to diagnose the region just beyond the cone (see diagram in Figure 15). Its report begins at 100 μs. One can see evidence of the wave moving up the cable over the next 150 μs. This cable also showed 6 measurements covering 50 cm of cable crush inside 100 μs near Box-A. These data were not plotted on Figure 15.

5. Note that on both K11 and K12 segments of record are shown on the early and the looped-back sections, and the wave velocities are about the same on each cable. But the apparent velocity of crush is more than a factor of two higher at the greater distance from the pipe in both cases. It is not clear why.

6. A shock propagation velocity in grout can be estimated from the time differences between reports at a given range beyond the cone on both K11 and K12. The results are 0.18 cm/μs from K11 in the interval out to 1 ft, and 0.19 cm/μs from K12 over the range from 1 to 2 ft from the pipe. These values are between the elastic wave speeds and the slowest plastic wave speed. They may provide a good estimate of pipe plasma pressure.

7. Note the K11 and K12 records in the interval from 0.2 to 0.25 ms and range from 7 to 8 m. Both cables were 26.5 cm from the cone; both were FSJ1-50. They should have reported simultaneously. There is a good chance they did. (But remember the observation of point 5 above. It may be relevant here.) If so, this apparent disagreement is an indication of a remaining calibration uncertainty. It may be related to the a small error in the propagation factor discussed above, or perhaps, to some non-linear variation of the propagation factor.

8. K11 showed a plasma interaction by 80 μs. This means the average first pulse velocity in the cone must have been greater than about 4.5 cm/μs. This value is uncertain because the end of the measured time is unknown within the pulsing interval.

Four cables were arrayed as large W's on each event. The resulting records from K5, K6, K7, and K8 on MISSION CYBER are shown in Figure 16. The first two were FSJ1-50 cable, K7 and K8 were "the weaker" RF-58 and RF-174. The first three show plasma interactions at the downstream point after 20 or 30 μs. This leads to LOS plasma velocity estimates between 4 and 6 cm/μs, values in good agreement with that noted above. Again, it will be difficult or impossible to improve these

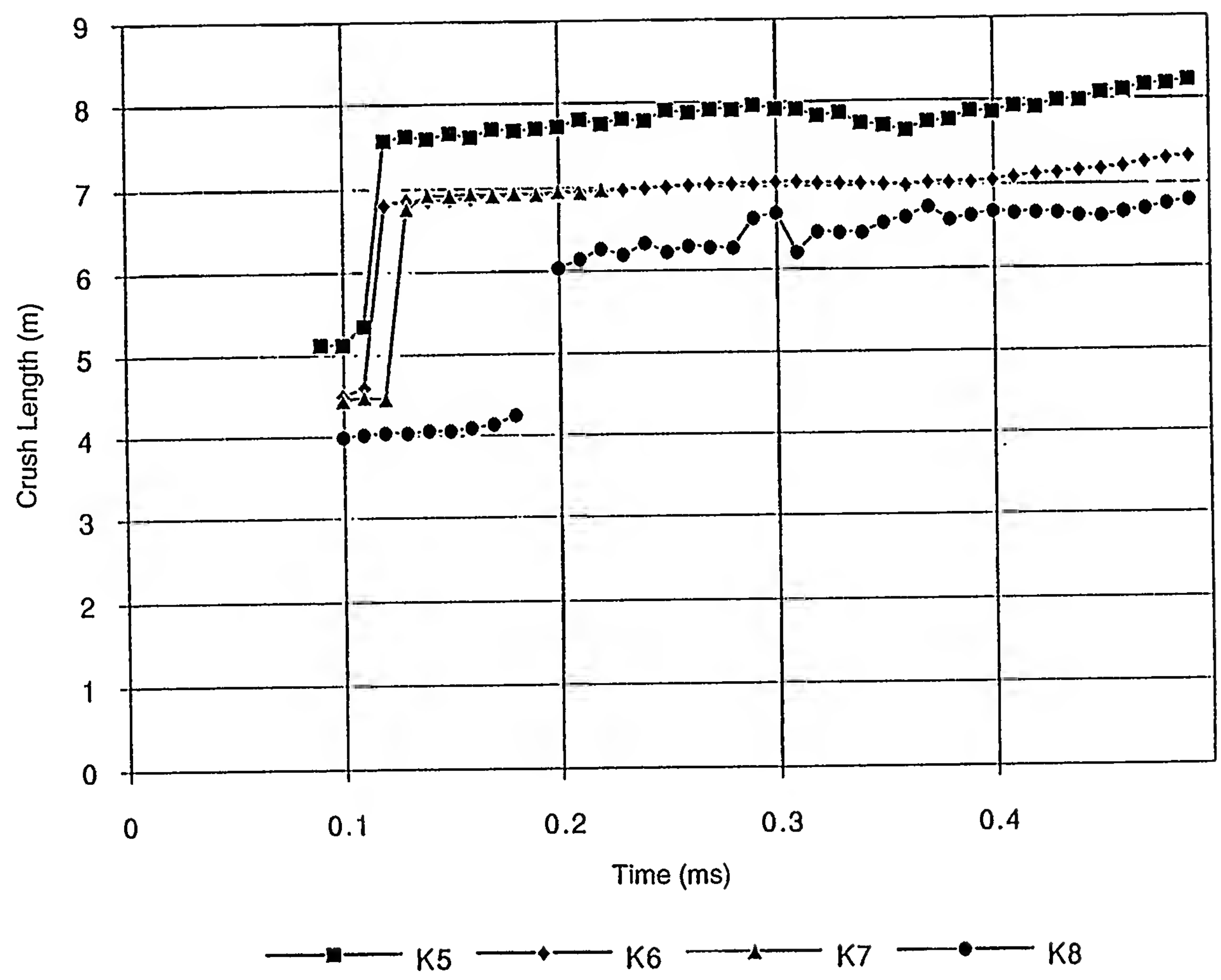

Figure 16. MISSION CYBER CORRTEX data from W-cables near reverse cone.

with that noted above. Again, it will be difficult or impossible to improve these numbers because of the relatively large pulse interval compared with the time being measured.

Note the record from K8. In this case the report at 6 m was delayed by about 60 µs. The apparent plasma velocity was only 1.2 cm/µs, clearly too low. This is further evidence that this presumably weak cable did not respond as weak when cast in homogeneous grout.

3.4 BOX-A REGION RESULTS.

Many cables were installed close to Box-A to diagnose early shock propagation. Rather little information was recovered in this region primarily because EMP upset and what is assumed to be cable damage from intense radiation wiped out much of the early data. There are bits of data from many places which can best be interpreted in comparison with detailed 2-D calculations of the close-in environment. Such comparisons have not yet been made. Several observation are listed below.

1. Four cables were installed perpendicular to the Box-A cylindrical surface. Only K7 gave data out to about 1 m. Data was received between 26 and 76 µs over the interval on the cable of 41 to 93 cm.

2. K4, K6, and K8 ran together about 1 m from the front corner of the box. K6 and 8 reported at about 20 µs which is much too early to be a shock-generated signal. K4 gave a more reasonable 80 - 90 µs response.

3. Cable K3 was routed behind and beside Box-A. It shows an early response which probably indicates an early shift in the center of the explosion into the void and sandbag region behind Box-A.

4. Cable K1 looped into a location only 2.24 m from the WP. An early report was seen on this segment starting at a range of about 2.6 m at 32 µs. The response point then drifted back toward the WP for 70 µs before moving out smartly under the influence of the ground shock. This is probably an indication of combined radiation and shock response.

4.0 DISCUSSION

It should be repeated that this is only a preliminary report of the partial results from a complex and elaborate experiment conducted on the MIDDLE NOTE and MISSION CYBER events. In particular, the CORRTEX data from MIDDLE NOTE has not yet been fully reduced by LANL, and that for MISSION CYBER is not finished. Some preliminary work done by the principal author on both events showed for MISSION CYBER that seriously flawed conclusions can be drawn from uncorrected data records. Therefore, very little has been said about MIDDLE NOTE

here. Also, the SLIFER data has not yet been carefully integrated with that from the CORRTEX systems.

It is clear, however, that useful data were obtained which will provide interesting tests of early-time calculations. The data are more limited than we had hoped, but it provides observations over much of the close-in region. Detailed comparisons between calculations and observations will begin when the data analysis is completed.

The CORRTEX records obtained along the LOS are fascinating in that they are systematically different than naively expected. The strange but consistent performance of the cable loops suggests that CORRTEX system response to cable distortion near the LOS is more complex than expected.

The loops show a response that is monotonic with cable length. The electronic system reports that a reflecting surface simply moves down the cable - sometimes moving toward the LOS, sometimes moving toward the center of the explosion. We all agree that this surface is not a shock front. Therefore, the loops clearly indicate that CORRTEX cables installed between about 8 and 25 m from the WP along the stemmed LOS tunnel for an event like MIDDLE NOTE or MISSION CYBER do not measure the location of the shock front. The interesting question is what causes the CORRTEX reflections and to what calculated quantity should the data be compared?

At least two suggestions have been made. The LANL experimenters believe the CORRTEX recorder is seeing a localized distortion of the cable which is adequate to reflect the sensing pulse. They argue that the advancing shock weakens the cable toward the WP. Even though an entire loop may be engulfed by an elevated pressure, the weakness causes a progressive failure to propagate down the cable. They do not argue that the cable is completely crushed as we naively imagine. Somehow the CORRTEX system can see through this damage occasionally to report an open cable many meters behind the apparent distortion front. One difficulty with this idea is that it does not explain the apparent speeding up of the signal as a loop is approached.

The principal author suggests an alternate explanation. It is based on the assertion that the CORRTEX cable installation was of too high a quality.

Most of the cables discussed above were buried in carefully controlled grouts. All of the cables except those in the free field were close to one or more pressure sources. Under these conditions the early ground motion was essentially hydrodynamic. The cables found themselves in a quasi-uniform pressure field which caused them to distort, usually not axisymmetrically, as governed by the stress field, the strength of the grout, and the strength of the cable. The cables did not short immediately after shock arrival. No doubt they did short at some later time, but that short probably had little if anything to do with the CORRTEX report on most cables.

A distorted cable segment would reflect some part of the CORRTEX pulse or represent some change in impedance to the SLIFER oscillator. When a long enough length of distorted cable was seen by the electronics, a closure was reported beyond the distorted region even if no short had occurred. The apparent closure moved ahead as the distorted length increased. It probably fell back relative to the shock front as the magnitude of distortion decreased as the range increased. The Brownlee knee is a well-recognized manifestation of failure of cable crush at the shock front.

Consider a loop region. The length of the distorted but not shorted cable was large compared with loop dimensions. At the loop there is two or three times as much cable per range station meter as on a simple cable run. When the extra cable is engulfed and distorted, the apparent crush point moves ahead correspondingly faster simply because the rate of integrated cable distortion has been increased threefold. This makes the "ground shock" appear to speed up as the loop is approached. Actually the shock front is well past the loop. Subsequently the shock continues along the regular cable run, and the crush point trails along behind. It doesn't care at all that the cable is in a loop configuration because the apparent closure is an electronic artifact not closely related to the initial shock wave.

For this explanation to be credible several conditions must be satisfied:

1. The length of the distorted cable must be longer than a loop. Consider Figure 14. The plasma pulse leads the apparent crush by at least 3 ms. The ground shock velocity is about 2 m/ms. The resulting 6 m length is much longer than a 1 m loop. Some loops near the WP also showed this peculiar behavior. Perhaps in some cases a precursor disturbance was generated by prompt radiation deposition in or near the cable. Such a disturbance might influence many meters of cable.

2. A partial reflection of electromagnetic energy from a distributed distortion of coaxial cable must be possible. This could be investigated by squeezing a long cable in a high pressure vessel or by flattening many meters of cable in some kind of long vise.

3. The CORRTEX electronics has to be capable of responding as suggested. This question needs to be addressed by knowledgeable experts.

It has been argued that the efforts to measure the shock environment near the WP failed because the emplacement was too good. The ground motion in carefully prepared grout is very near that calculated. On the other hand, it is well known that geologic formations are not uniform. There is much experimental evidence that ground motion in the strong shock domain of interest for yield determination is dominated by generalized block motion. When a cable crosses such a motion interface, the highly localized distortion shorts or opens the cable, and a good CORRTEX or SLIFER report is generated.

On the two events of interest here, the CORRTEX cables were dressed along the WP side of the bypass tunnel wall. Presumably the block motions in the wall propagated far enough into the grouted tunnel to influence the diagnostic cables as expected.

In most applications of these techniques for yield determination the cables are placed in non-radial bore holes and surrounded by minimal amounts of grout. They should, and do, respond well under those conditions.

A more complete discussion of close-in block motion and some of its possible implications can be found in the paper "Thoughts On Containment" by the principal author presented at this symposium.

Both of these suggested explanations leave unanswered the basic question of what these diagnostic systems are seeing. The agreement between K4 and K6, for instance, strongly suggests that something identifiable is involved.

5.0 RECOMMENDATIONS

First and foremost, of course, the analysis of data from these two experiments should be completed and reported. This will involve the comparison and consolidation of CORRTEX and SLIFER data.

If there is interest in exploring the phenomenological questions raised above, the first thing to do is to understand in detail how the CORRTEX electronics works in the environment postulated here. If it could respond as first proposed, determine how the progressive cable collapse can occur. The second suggestion would required laboratory work to see what cable distortion over what cable length gives what CORRTEX response. One might then ask if such distortions are credible in the nuclear test environment.

An interesting experiment would mount a CORRTEX cable on something like a heavy chain that would introduce large impedance discontinuities every few centimeters. Such discontinuities might cause prompt cable failure soon after shock arrival, and system response, including loops, might be as expected. Certainly discrete pressure gauges should be mounted at several locations along the sensing cable.

Another interesting experiment would compare two CORRTEX free field measurements. One would be made as in these events and one would use a cable hung in the center of the bypass drift where block motions might be modified but probably not suppressed by a meter or so of uniform grout.

Cavity Conditions

The Size of Underground Cavities Formed by Nuclear Devices: Implications of Point-Source Solutions for Geological Materials

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Abstract

Containment depends on knowledge of the size of a cavity created by a given event. Estimates are determined by the CEP equation or other scaling laws, and by prior experience. Scaling laws can be determined by point-source solutions for appropriate geological materials.

The scaling of explosive events in geological materials is much more complicated than for explosions in a perfect gas, as in air blast analyses. In this latter case, the scaling has been known since the early point-source results of G. I. Taylor, Sedov, and Von Neumann in the 1940s and 1950s. Those results are based on the assumption that the energy E of the source determines all subsequent phenomena at all ranges large compared to the actual device dimensions, and at times large compared to the detonation time. That assumption leads to specific forms such as the common $p \sim Er^{-3}$ pressure decay, $v \sim E^{1/2}r^{-1.5}$ particle velocity decay, $R \sim E^{1/5}t^{2/5}$ shock propagation distance, and so forth. Sedov gives exact analytical equations for the entire flow field behind the shock for this point-source solution, as a function of the perfect gas constant γ. This solution is so well known that it is often referred to as "*the* strong shock solution."

Less well known, but of more importance for geological materials are the point-source and "strong shock" solutions for perfectly porous materials given by Kompaneets. These are materials for which any small initial pressure compresses them from an initial density ρ_0 permanently to some final density ρ_1, and subsequent behavior in both loading and unloading is incompressible. The scaling for these materials is determined by an exponent μ determined explicitly from the porosity $k = 1 - (\rho_0/\rho_1)$. Depending on that porosity, the pressure can decay with range to an exponent ranging anywhere from –3 to –6.

There are also point-source solutions for more general material models; these have recently been obtained by the author; these are material models with not only a porosity, but also a finite rather than a zero compressibility after compression. The scaling laws for these materials has been obtained in terms of a single exponent that determines all aspects of the point-source solution. That exponent depends on both the porosity and the compressibility. The limiting cases of this theory are the perfect gas limit when the porosity goes to zero, and the perfectly porous case when the compressibility goes to zero.

A discussion and complete results for these point-source solutions, and the resulting scaling laws will be presented. The application to the cavity formation problem will give scaling laws with a material property dependence. The implications with regard to cavity size scaling will be discussed. A specific form like the CEP equation, but with exponents depending on material properties, and especially on the porosity, is obtained.

GALENA Pressure History and a Proposed Sensor for Mean Residual Stress

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Abstract

A new, light-weight, inexpensive cavity pressure system was fielded on the GALENA event. This system included a high pressure liquid pump (2500 psi, 1.5 ml/min.), a high-pressure hose extending from the instrumentation package to about 100 feet above the working point, and a stainless steel capillary tube extending from there into the expected cavity region. The system was pressurized to about 1500 psia before detonation. The resulting data suggest pressure equilibrium with the cavity region was reached within 10 s of detonation, and the subsequent pressure history, ranging from above 500 down to 40 psia, appears to be a valid measure of cavity pressure

The data acquired before 1 s suggest that the high-pressure hose performed as a pressure sensor as the initial shock wave passed over it. We plan to use a system similar to the one described above, without the capillary tube extending to the working point region, to sense mean residual stress on future events.

Introduction

The cornerstone of modern containment mythology has, for the past twenty years, been the "belief" that a residual, compressive stress sets up in the zone between one and two cavity radii from the working point as a result of the explosion dynamics. The condition of high residual transverse ("hoop") stress, as compared to pressure inside the cavity, is believed to be an important (if not key) factor in effecting containment. As a result, significant efforts have been expended to give assurances that the surroundings of a proposed test are consistent with those giving rise to a favorable ratio of cavity pressure to residual stress.

The word "belief" is used because the evidence for either the existence or the effectiveness of residual stress is mostly circumstantial. Since direct confirmation (or denial) of the beneficial effects of residual stress requires measured values, several attempts have been made to measure the radial and transverse components of residual stress, with very limited or questionable success. Primary problems involve cable or gauge survival, and the effects of encapsulating media, e.g., grout or epoxy cement.

Cavity pressure history, from a few seconds after detonation to collapse, was first measured on the LLNL CORNUCOPIA event in 1986. Since then, the LLNL has successfully measured cavity pressure history on five events, the most recent of which was GALENA. GELENA used a new system, illustrated in Figure 1, designed to be much easier to field, safer, and less expensive.

The cavity pressure measurement on GALENA

The high pressure hose depicted in Figure 1 was about 58 m long, with a stainless steel (SS) extension of about 13 m, and was pressurized with a mixture of water and ethylene glycol to about 1500 psia pre-shot. The pressure transducers were rated to have a linear response to 3700 psia and were claimed by the manufacturer to be undamaged to about 10,000 psia. The lower end of the tube was placed about 5 m from the device and was expected to be well within the vaporization region of the explosion. It was expected that the high pressure liquid within the tube would maintain an open path through the tube once the lower end was burned or melted off. About ten minutes after detonation, the pump was activated for around one minute to determine whether the tube remained open: no pressure change was registered, indicating that the tube was open.

The resulting pressure histories are shown in Figure 2 and compared to the those from other events in Figure 3. While these data show qualitative similarities, information from more events are needed before a suitable predictive model can be developed. Such data should include a variety of site characteristics, yields and burial depths.

The GALENA data suggest that direct comparison of pressure in the cavity and residual mean residual stress outside of the cavity may be accomplished with a system similar to that used on GALENA. The upper plot in Figure 2 shows data recorded during the first two seconds after detonation. The first 100 ms of pressure history shown in Figure 4 suggests that, after the SS tubing is burned or melted open, the pressure in the system begins to bleed off and reaches equilibrium with pressure in the cavity. However, when the strong shock (many kilobars) reaches the high pressure hose (which is much more easily compressed than the SS tubing), a signature of this high pressure is transmitted to the gauges. The arrival time of the high pressure pulse is consistent with that predicted for the sensor location. After ground shock has passed beyond the high pressure hose, the decreasing pressure history results.

Proposed sensor for mean residual stress

These data further suggest that, if the pressure were not allowed to bleed off through an open tube, the high pressure hose could be used as a sensor for monitoring the formation and decay of residual mean stress. A modification of the GALENA system to measure the residual mean stress is shown in Figure 5. Figure 6 illustrates the system we plan to field on future events for providing a direct comparison between cavity pressure and residual mean stress in the region of the deep plug.

Work performed under the auspices of the U.S. Department of Energy by the Lawrence Livermore National Laboratory under Contract W-7405-Eng-48.

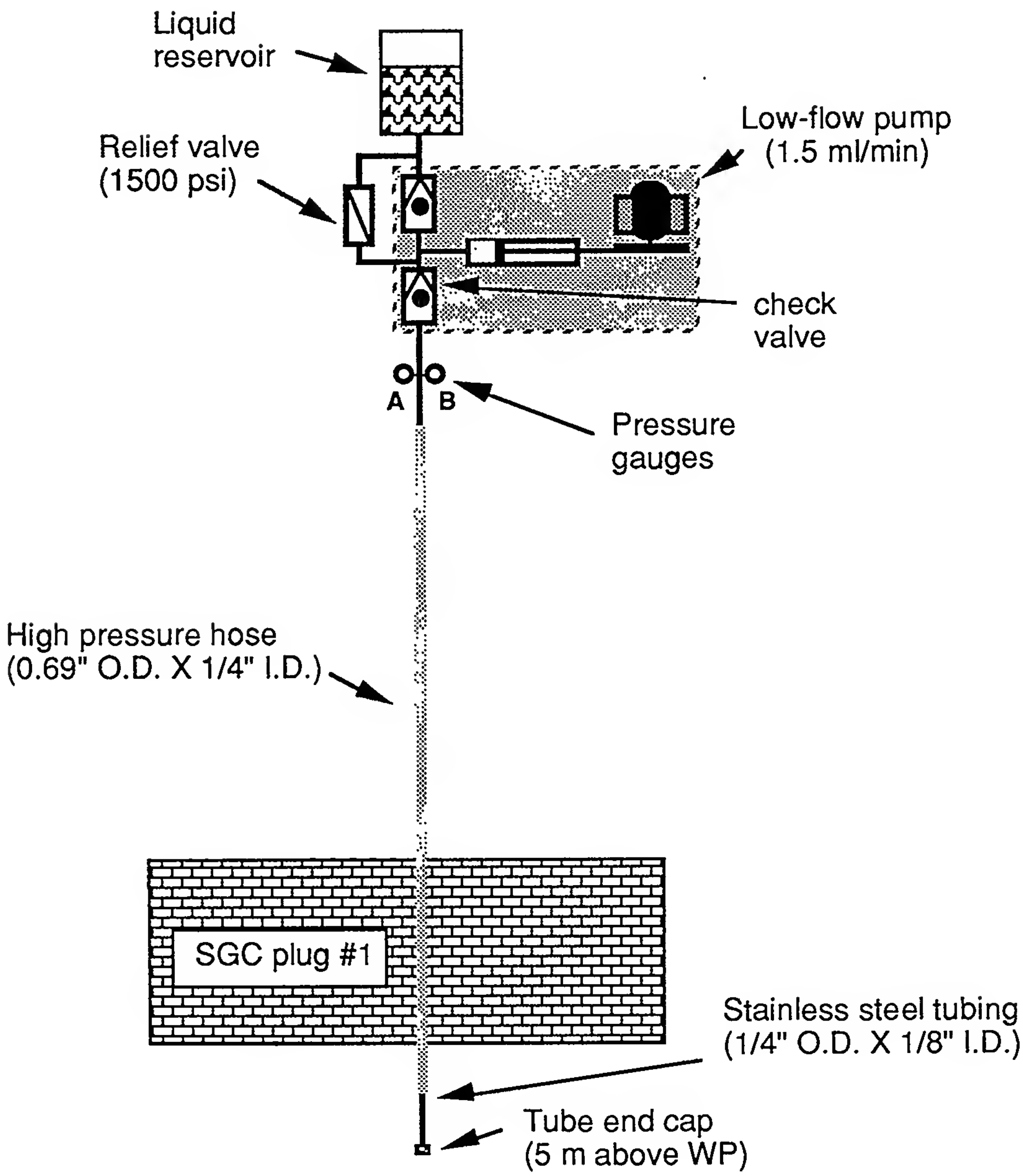

Figure 1 GALENA cavity pressure measurement system. The high pressure hose and stainless steel tubing were filled with a mixture of water and glycol before installation. The system was then pressurized to about 1500 psi.

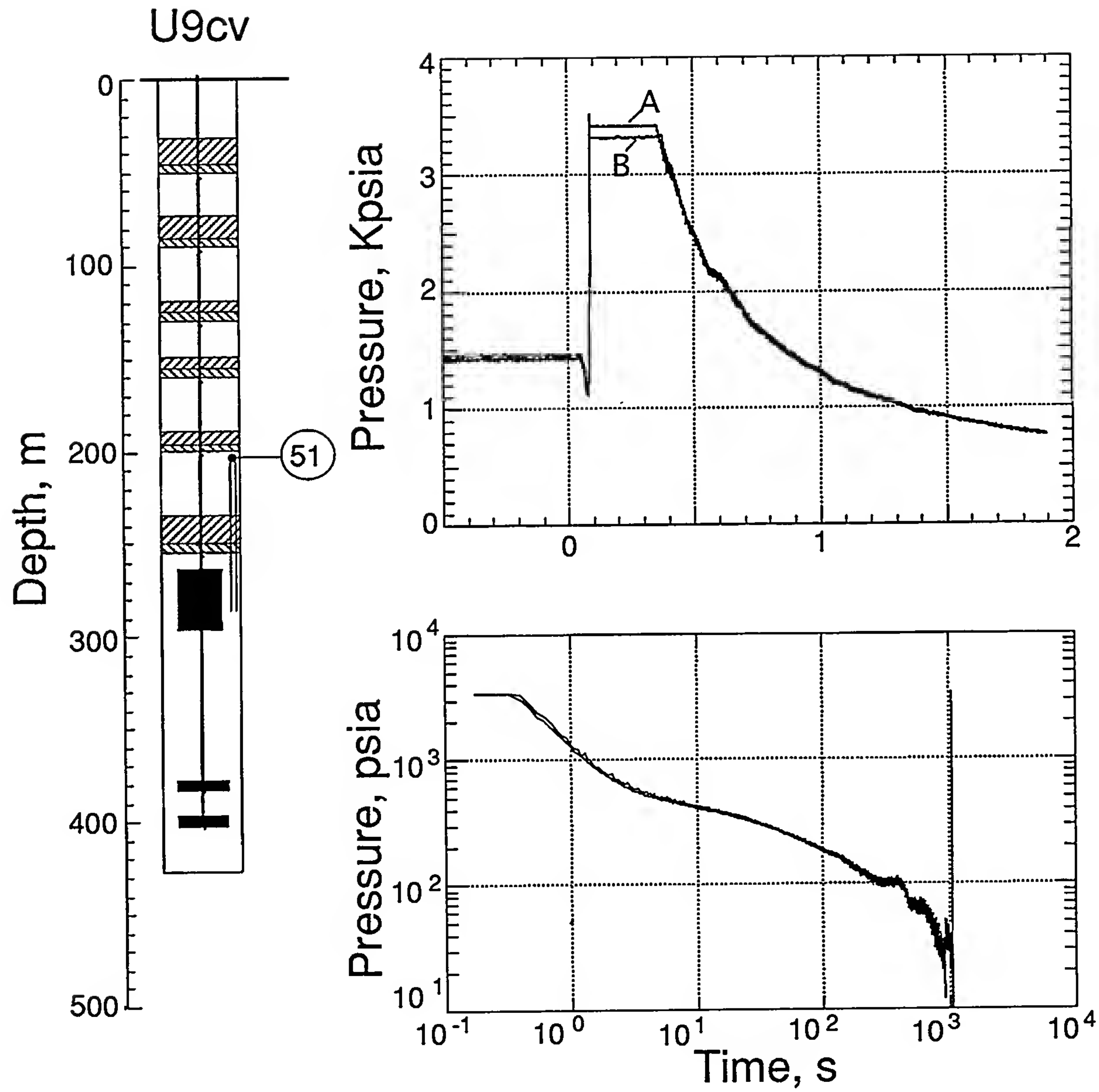

Figure 2. GALENA cavity pressure history. The initial plateau indicates the pressure was greater than the recording system range earlier than about 0.4 s.

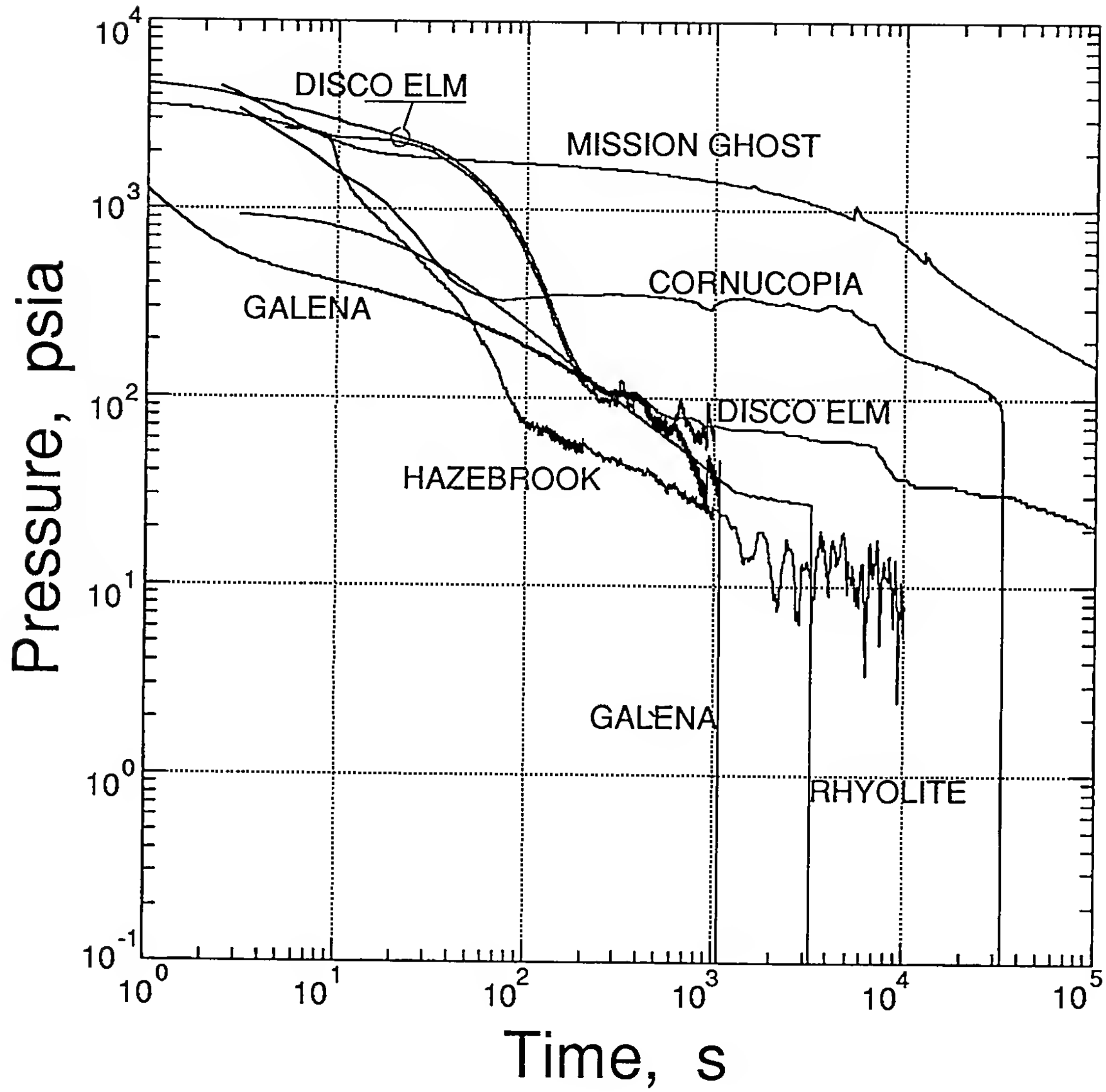

Figure 3. A comparison of GALENA cavity pressure history with those of other events.

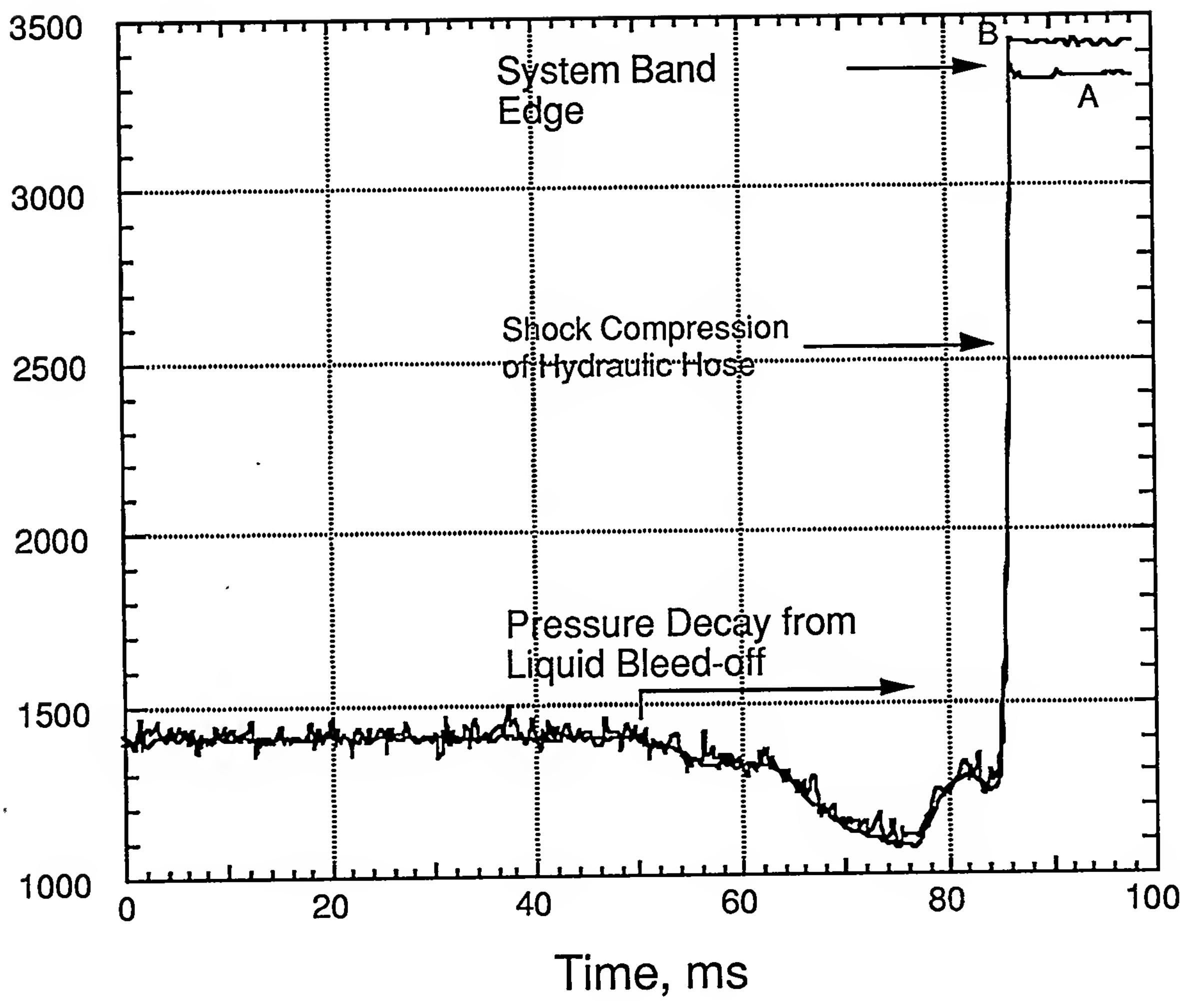

Figure 4 GALENA early time cavity pressure history.

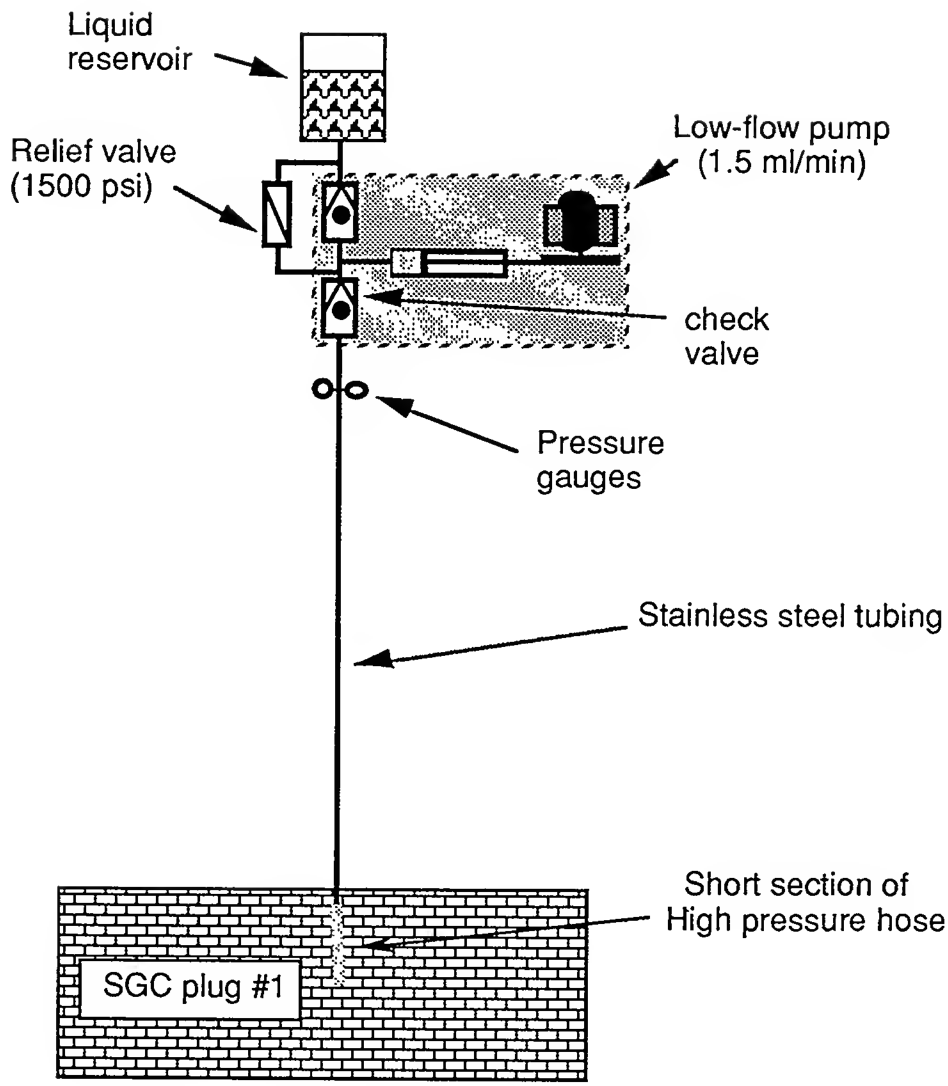

Figure 5 Proposed residual mean stress monitoring system

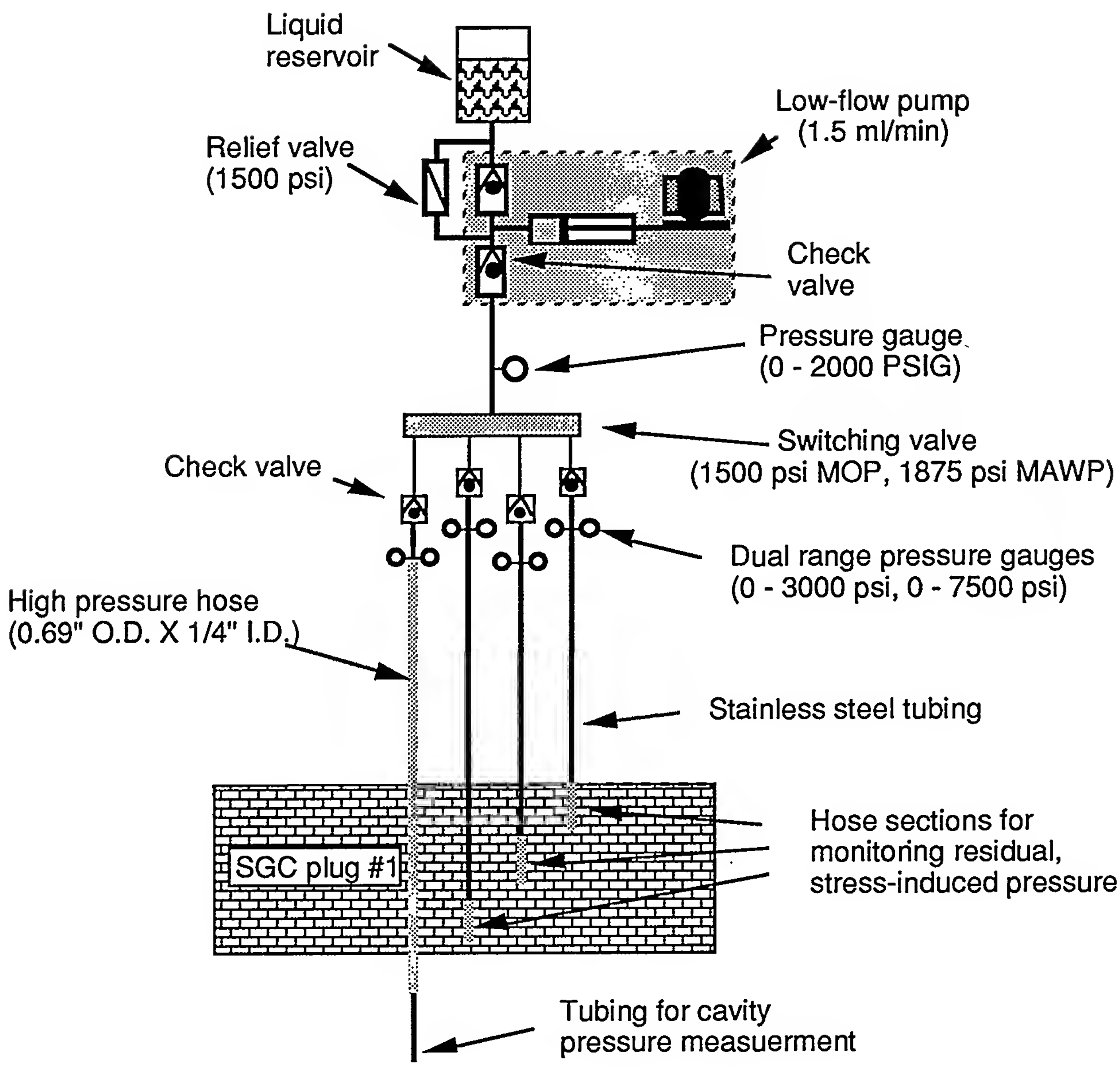

Figure 6 Cavity pressure and residual stress-induced pressure monitor planned for future events. Hose sections for monitoring residual stress-induced pressure will be raised to overburden pressure after emplacement of the SGC.

CAVITY GAS PRESSURE MEASUREMENTS ON DIAMOND FORTUNE

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ABSTRACT

Five measurements were made of the gas pressure in the cavity as a function of time on the DIAMOND FORTUNE event. The measurements were obtained with a fast and slow blowdown technique; an additional transducer on the cavity side of the explosive valve provided valuable data. Data from three of the five transducers are nearly coincident; data from the other two show temperature-related shifts. The timeframe of the credible measurements extends from 0.1 second to four hours. The pressure at two seconds was 2 100 kPa (300 Psi). On the logarithmic time scale this level decays monotonically to 210 kPa (30 Psi) at 1000 seconds. During the next decade of time the cavity pressure approached atmospheric pressure. Over the initial few seconds the measured amplitude is close to the S-Cubed prediction; with increased time the measured pressure decays at a faster rate.

INTRODUCTION

Briefly, the DIAMOND FORTUNE event consisted of a small nuclear device centrally located in a large underground cavity (P Tunnel, Nevada Test Site). The ratio of explosive yield to cavity volume was sufficiently low that the cavity walls were predicted to respond elastically, that is, it was a so-called decoupled event. A few milliseconds after detonation the shock waves in the cavity gases decay. Pressure and temperature of the cavity gas then decrease with time as a result of energy loss mechanisms. Although potential loss mechanisms, such as vaporization of water in the walls, thermal conductivity, etcetera, are understood, the dominant one - or ones - for an underground cavity have not been fully identified. Measurements of cavity gas pressure, along with cavity gas temperature, are needed to compare with predictions that are based on assumed energy loss mechanisms. (Another paper in the symposium describes the temperature measurements.) Additionally, if cavity gas leaks through the geologic host or man-made hardware into the tunnel complex, the cavity pressure measurements are useful as a source term in a subsequent analysis.

On this event our objectives were: measure the cavity pressure within a second of detonation and to a time when the pressure approached the atmospheric level, obtain multiple measurements to increase credibility, and, finally, compare these measurements with predictions and measurements on previous events.

TECHNIQUE

Cavity pressure was measured with a so-called blowdown technique; the technique has been successfully used on previous events. The hardware consists of a blunt nose, steel pipe extending from the cavity wall to an explosive valve and a pressure reservoir 2 to 10 meters away. Pre-event the reservoir is pressurized to a level well above expected cavity pressure. In the second before zerotime the explosive valve is fired allowing flow of gas through the pipe and out the cavity end. The intent is to keep debris associated with the impinging air shock from entering and clogging the pipe. Within a second for the 10 meter pipe, and a hundred milliseconds for the 2 meter pipe, the pressure in the reservoir comes to equilibrium with the pressure in the cavity. A commercial transducer in the reservoir senses the pressure.

THE EXPERIMENT

Figure 1 shows the slow blowdown hardware used on DIAMOND FORTUNE. The 10 meter long pipes were grouted into radial holes drilled from an adjacent drift into the cavity wall. In the

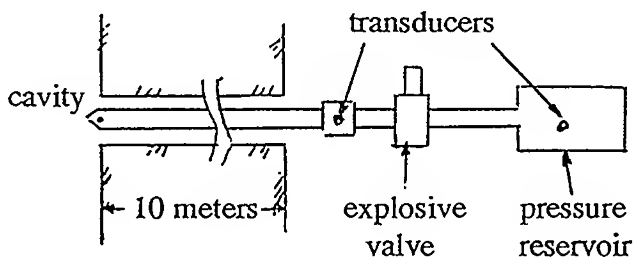

Figure 1. Slow Blowdown Scheme fielded on DIAMOND FORTUNE.

drift there was a pressure transducer mounted in a pipe coupling. Next was the explosive valve and then the quarter liter pressure reservoir. The latter contained a second transducer. The fast blowdown scheme involved a 2 meter long pipe; the short length allows the assembly to reach equilibrium with the cavity pressure quicker than the slow scheme. A single transducer was used in the fast scheme. It was mounted behind the explosive valve in section of pipe that served as a pressure reservoir.

Figure 2 shows a plan view of the DIAMOND FORTUNE cavity and surrounding drifts. The 10 meter holes for the two slow blowdown pipes were drilled from the RUN AROUND Drift; explosive valves and reservoirs for these experiments (4700 and 4705) were also in the RUN AROUND Drift. Figure 2 also shows the location of the fast blowdown experiments (4710 and 4715). These assemblies were installed in the upper left corner of the bulkhead that covered the cavity access drift. Both of these locations were subsequently stemmed. Table 1 shows the transducer numbers, blowdown type, and transducer location.

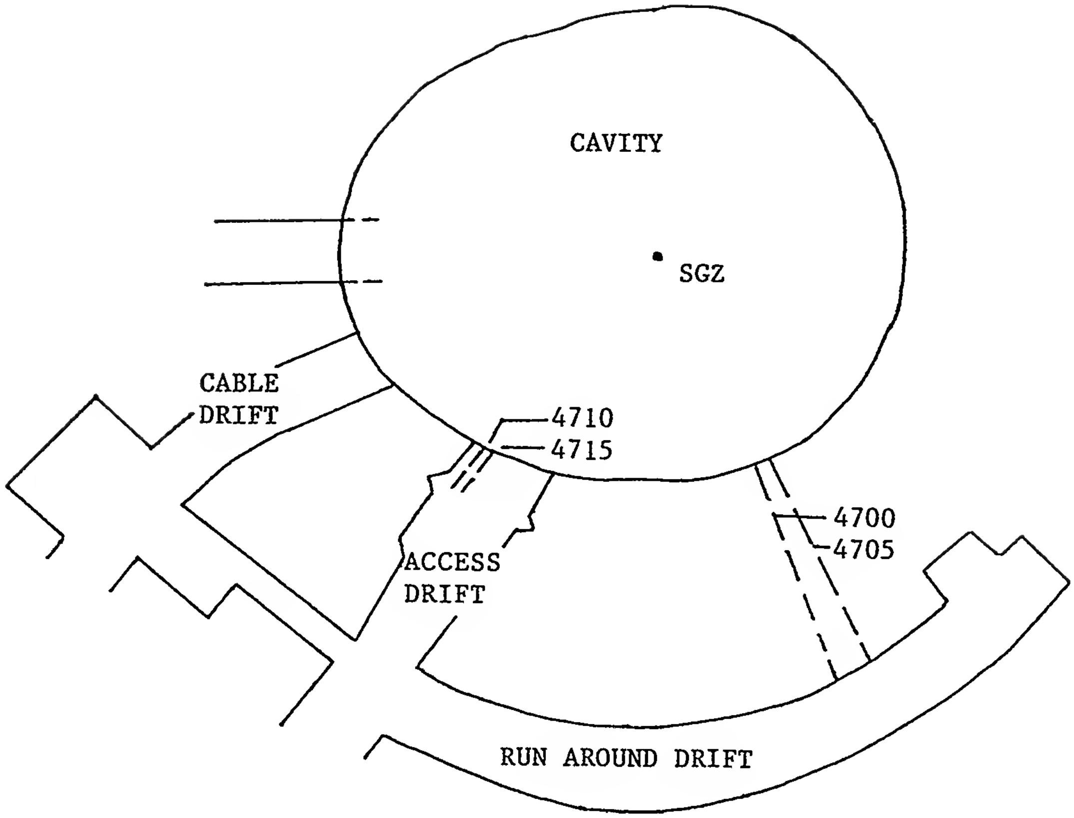

Figure 2. Plan View DIAMOND FORTUNE Cavity and Access Drifts

Table 1. Transducers for Fast and Slow Blowdown Schemes

DNA #	Blowdown Type	Reservoir Pressure	Transducer Location
4710-01	fast	1 Kpsi	reservoir
4715-01	fast	2 Kpsi	reservoir
4700-01	slow	--	coupling
4700-02	slow	10 Kpsi	reservoir
4705-01	slow	--	coupling
4705-02	slow	5 Kpsi	reservoir

MEASUREMENTS

Pre-event the transducer in the 4710 fast blowdown pipe failed. Also, the 4700-01 transducer in one of the slow blowdown pipes showed a significant thermal response when subjected to the heat from the curing of the stemming grout. We suspect the temperature compensating network in the transducer was lost.

Dry runs after stemming revealed additional problems with the slow blowdown hardware. The transducers in the two reservoirs showed a decreasing pressure. We had experienced this effect on previous events and suspected metal shavings in threads or residual machining oils in threads.

During assembly of the DIAMOND FORTUNE hardware we carefully cleaned the threads and followed manufacturers torque recommendations. A credible explanation is that differential expansion occurred between the 4130 steel pipes and the mild steel reservoirs. The fast blowdown scheme, however, retained its pressure. Its hardware contained only stainless and 4130 steels.

Within the second before event detonation the explosive valves were fired. Credible data was obtained from the fast blowdown transducer and from three of the slow blowdown transducers. Figure 3 shows 10 seconds of these data. The lower three traces came from the slow blowdown transducers. The pressure rise in the zero to one second timeframe is the response of the 10 meter pipes to the incident air blast wave. We believe these gages are measuring cavity pressure beyond 1 second. There is about a 15 Psi variation between these 3 measurements. In part this is associated with using 5 000 and10 000 Psi gages at 300 Psi. (The variation is also within the manufacturers linearity and hysteresis specifications.) The upper trace in figure 3 came from the fast blowdown pipe. We suspect the heat from the curing grout caused a zero shift of the transducer. Since the reservoir was pressurized pre-grouting, we had no way of rezeroing the gage after the grout cured and before detonation. As seen in figure 3, the fast trace shows 44 Psi above the average of the three slow gages. At the end of data recording - 240 minutes - the fast blowdown trace showed a 35 Psi offset above zero pressure; the three slow blowdown traces showed zero pressure. The closeness of these numbers - 35 and 44 - suggests that the thermal shift interpretation is plausible. The 9 Psi difference may be a temperature effect. In a subsequent paragraph we will discuss the early time oscillations seen in figure 3. Data points that comprise this figure are 134 microseconds apart.

Figure 4 shows 600 seconds (10 minutes) of data from the three transducers in the slow blowdown hardware. We see that the cavity gas pressure has decayed to 50 Psi by 540 seconds (9 minutes). These data were obtained with a data logging scheme and are spaced one second apart.

Figure 5 shows 240 minutes of data from the 4705-01transducer in the slow blowdown assembly. (A digital filter has been used to smooth the data.) Data from the other two transducers are virtual overlays as seen in the earlier figures. Here we have used a logarithmic scale for the time axis. The pressure decays linearly on this display to about 30 Psi at 1000 seconds. Over the next decade of time the rate slows as the pressure approaches zero or atmospheric pressure. (The thickness of the trace at late times reflects the numerous data points - spaced one second apart - and their plus and minus noise.)

Returning to the fast blowdown data shown earlier, we show the early portion of these data in figure 6. Although the figure shows 1 to 10 000 milliseconds, the correct time span is minus 9 to 9990 milliseconds. We have added 10 milliseconds to the time base in order to avoid the logarithm of zero. The solid trace is the event data. We see the preshot reservoir pressure of 2950 Psi, a rapid drop after the explosive valve was fired, and notable detail before the trace settles at 1000 milliseconds. The dashed line is data from a preshot, dry run of the fast blowdown system. In that test the pipe blew into atmospheric pressure in the cavity. We see a reservoir pressure of 2 200 Psi, a rapid drop after the valve is fired, an inflection and plateau, and then the pressure decays to zero within 100 milliseconds. Thus, 100 milliseconds is the response time of this hardware. Data from the shot, then, can be interrupted as cavity pressure beyond 100 milliseconds. Returning to the shot trace, we see the drop of pressure after the valve is fired and an inflection and plateau like the

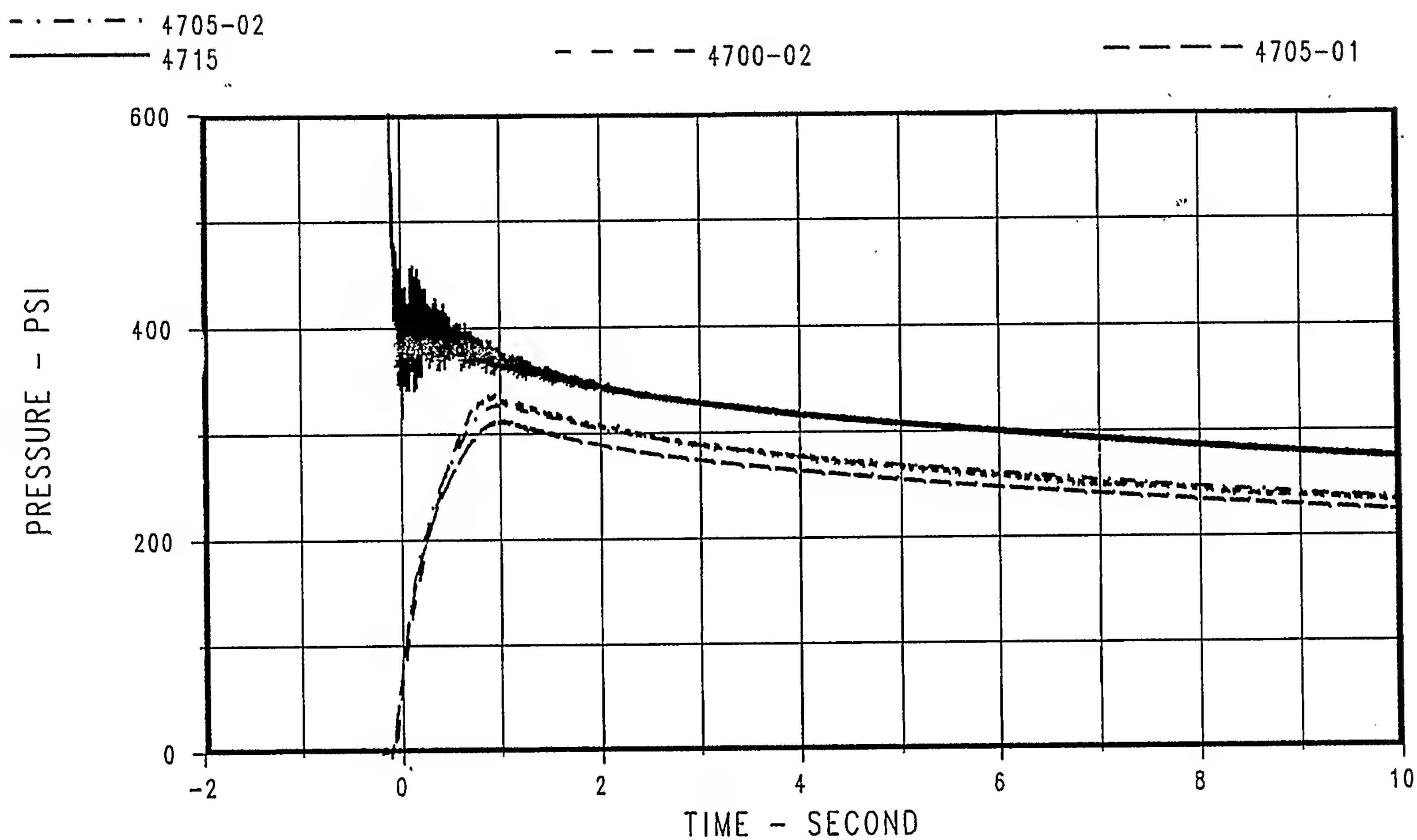

Figure 3. Ten Seconds of Data from the Fast and Slow Blowdown Transducers.

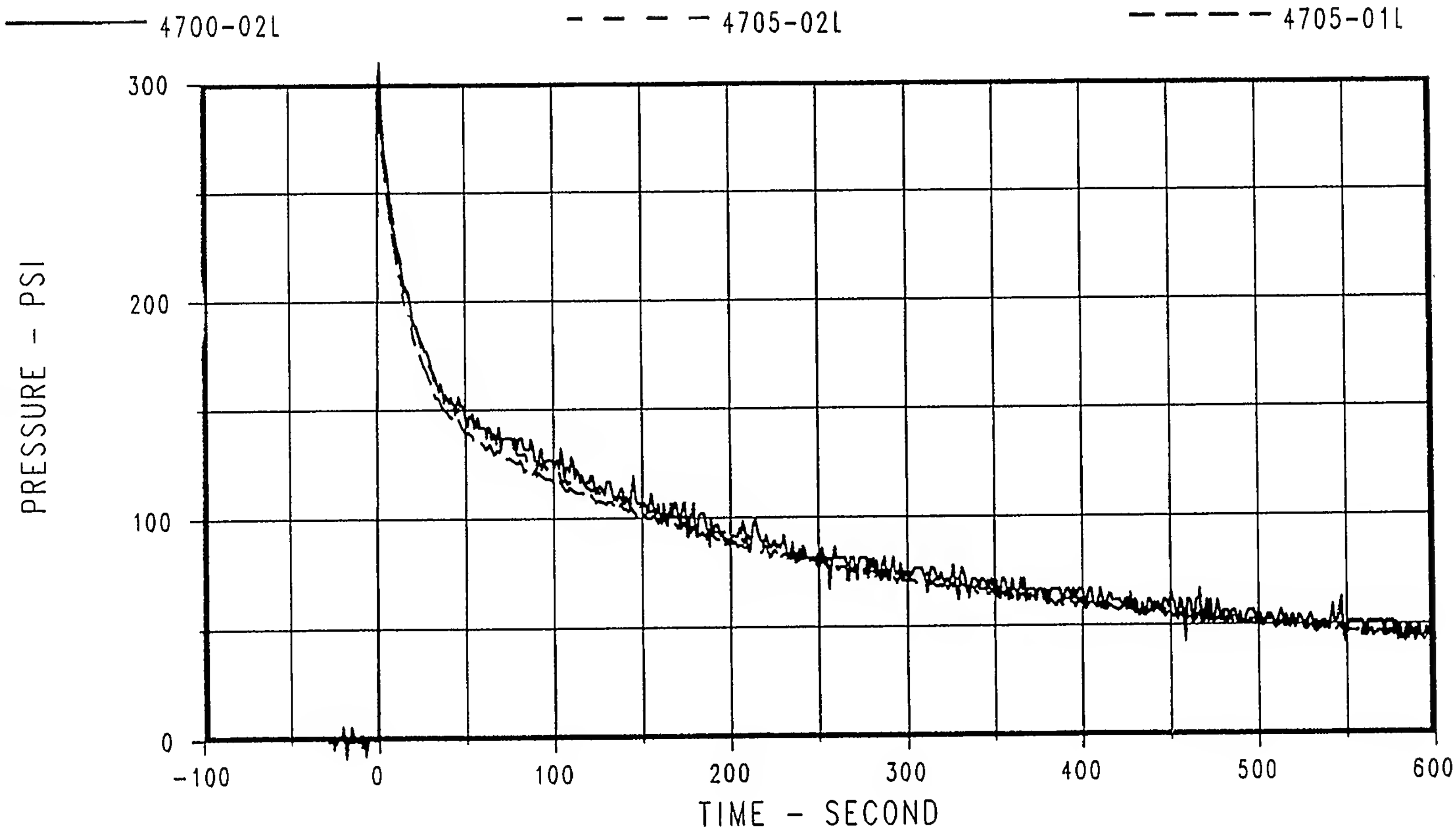

Figure 4. 600 Seconds of Data from Three Slow Blowdown Transducers.

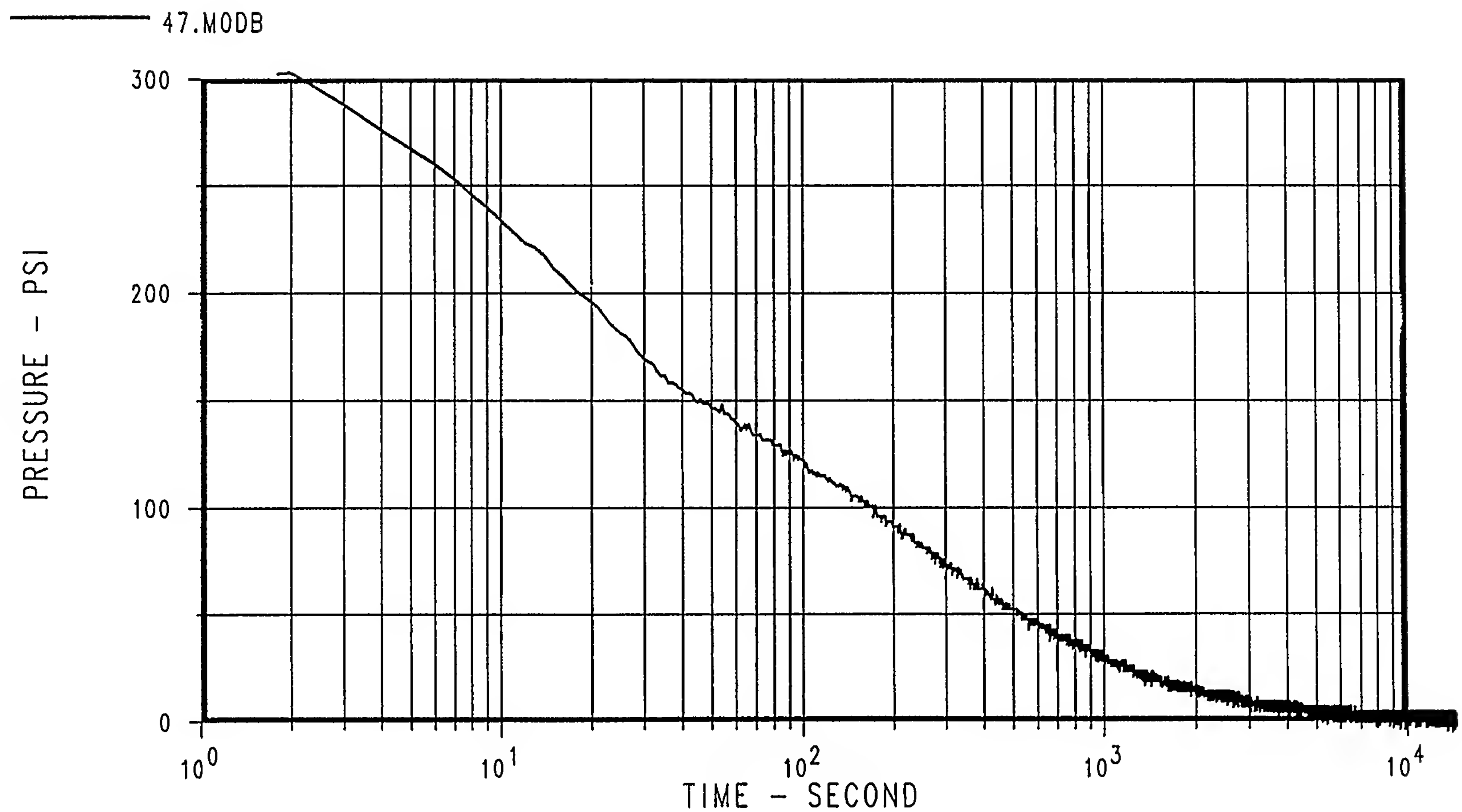

Figure 5. 240 Minutes of Data from 4705-01 Slow Blowdown Transducer.

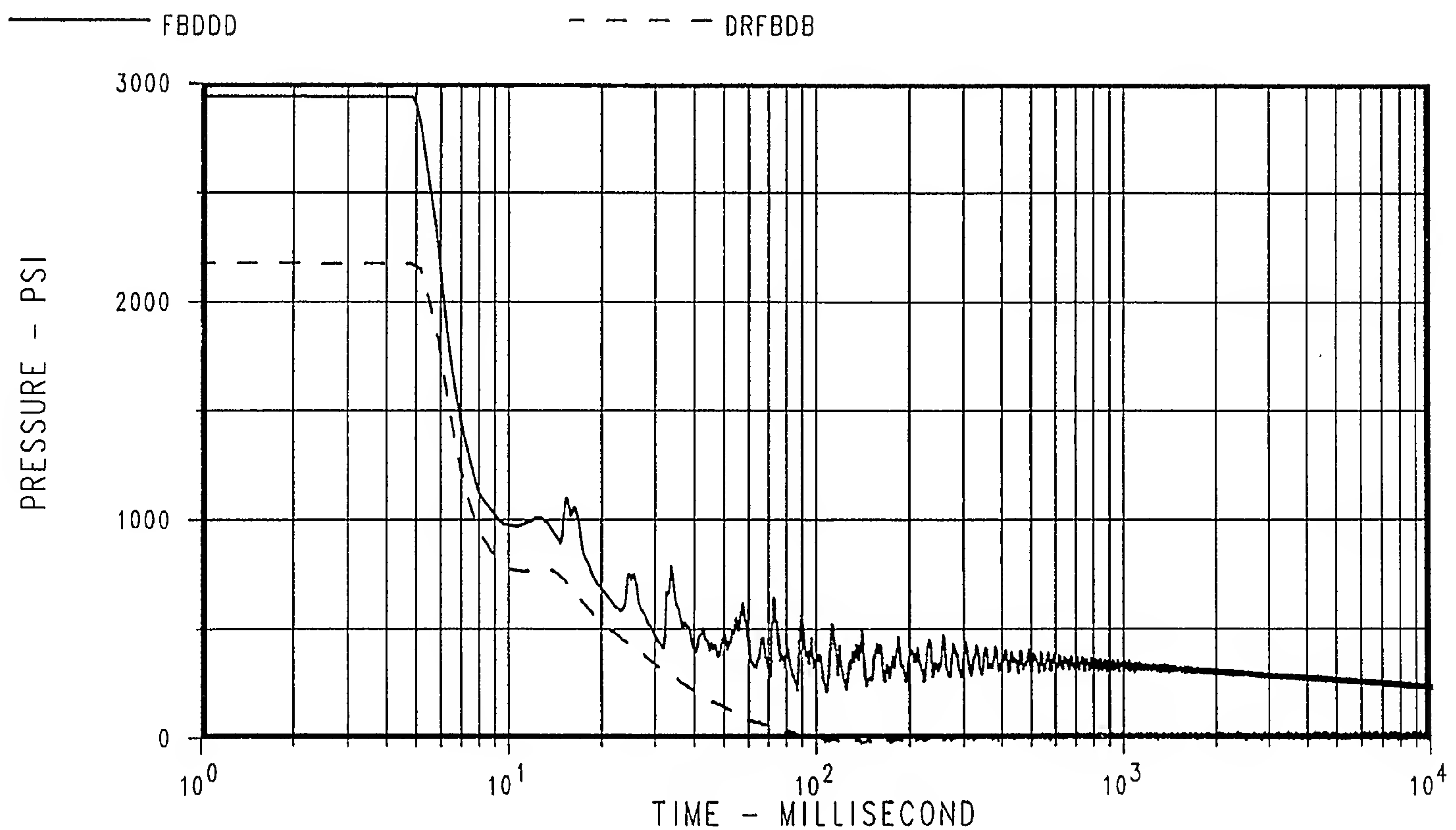

Figure 6. Early Time - Fast Blowdown Event and Dry Run Data.

dry run data. At 15 milliseconds - 5 milliseconds in unshifted time - we see a signal arrival followed by two more at 24 and 32 millisecond. Then there is a series of oscillations with some higher frequency content. The timing of the first signal suggests that it is the arrival of the air blast wave that has traveled through the 2 meter pipe. The timing of the two subsequent signals is consistent with two round trips of the first signal in the pipe. The explanation of the subsequent oscillations is less clear. We know, however, that an organ pipe resonates when its length is a quarter wavelength. Using 2 meters for the length and an acoustic speed of 340 meters/second, we calculate a resonant frequency of 42 hertz. The oscillations beyond 80 milliseconds show a frequency of about 37 hertz. This analysis suggests that the blast wave has induced a resonant response in the fast blowdown pipe.

Even if the above analysis is not a correct interpretation, we know from the dry run data that the measured pressure reflects cavity conditions after 100 milliseconds. Bounds on the oscillatory pressure at 100 milliseconds suggest a pressure of 256 to 356 Psi. (We have subtracted the 44 Psi offset discussed earlier.) This pressure level persists to about 600 milliseconds at which time we see the start of the pressure decay.

COMPARISONS

In figure 7 we again show the data from one of the slow blowdown experiments on a logarithmic time scale. The dashed line is the S-Cubed prediction for DIAMOND FORTUNE. There is approximate agreement at early time; later the measured pressure decays quicker. We suspect cavity pressure was lost into fractures or porous zones in the walls of the mined cavity. DNA has conducted previous events with the same type of nuclear source and in similar size cavities. The late time MINI JADE data, shown in figure 7, was a DNA measurement on that event. MINI JADE was sited in the low permeability tuff of N Tunnel. The pressure decay parallels the S-Cubed prediction for DIAMOND FORTUNE. The short, thick trace is a Sandia measurement on MILL-YARD. Again, it shows pressures comparable to DIAMOND FORTUNE and its predictions at early time, but then decays rapidly. Although this event was also conducted in N Tunnel, the lower portion of the mined cavity had been filled with Yuma dirt. We suspect that flow of gas into the porous dirt invert rapidly reduced the cavity pressure.

CONCLUSIONS

We interpret the fast blowdown measurement to show 256 to 356 Psi at 100 milliseconds. This level persisted to about 0.6 seconds and then began to decay. The three slow blowdown measurements show an average of 322 Psi at one second. These decay monotonically as the logarithm of time to abut 1000 seconds. The decay rate then slows and pressures close to atmospheric were reached about 10 minutes after zero time. Measured cavity gas pressure is comparable to the S-Cubed prediction at early times but decays faster. We attribute the quicker decay to losses through geologic fractures or porous zones.

This work was performed by Sandia National Laboratories under contract DE-AC04-76DP00789 with the U.S. Department of Energy.

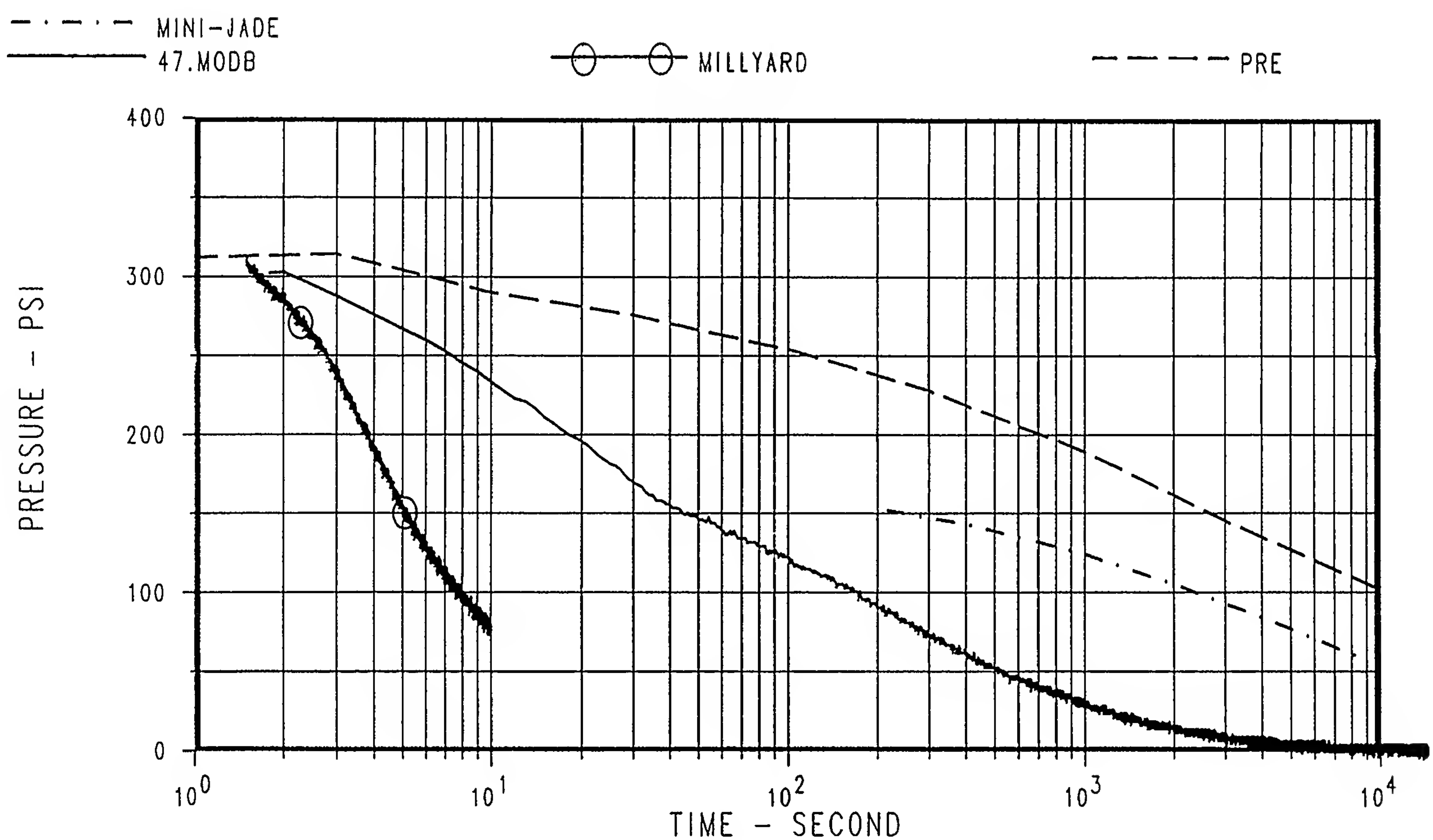

Figure 7. Cavity Pressure Prediction, DIAMOND FORTUNE Data, and Previous Event Data.

LYNER and Chemical Kiloton

Containment and Safety Data Acquisition System for the LYNER Complex

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ABSTRACT

LYNER is an underground tunnel complex constructed near the U1a shaft designed to perform weapons related tests. A number of measurements are planned to obtain containment, environment, and safety related information. Unlike Los Alamos vertical weapons tests where data collection is performed from a trailer at the surface, containment related data collection instrumentation will be installed in an underground alcove at one end of the complex. We describe the measurements we plan to make, how we expect to make them, the hardware involved, and the process by which the selection was made.

I. INTRODUCTION

LYNER is a tunnel complex located under Yucca Flat for the purpose of conducting weapons related tests. It is composed of a 1200' long main tunnel constructed 1000' below the surface and two vertical shafts to the surface, one at each end of the main tunnel. The shaft at the south end of the main tunnel (U1a) is the main entry point, while the stemmed shaft at the north (U1g) serves as the downhole pathway for the cables and as an emergency exit through an open cylindrical tube with a ladder. Two experimental alcoves are located near the U1g end of the complex. Individual experiments will be conducted in grouted side drifts running off the main tunnel.

During the LYNER feasibility study we decided that personnel time downhole could be minimized by locating the data collection hardware as close to the sensors as possible. Once setup was completed monitoring and

control of the downhole instrumentation could be transferred to a more hospitable location.

The use of the LYNER complex is still in many ways undefined. Thus, a primary constraint of any data collection system is that it must be sufficiently flexible to accommodate any expansion or modification to the amount and type of data we expect to obtain.

With these considerations we first examine the measurements we currently expect to obtain, the requirements we imposed to guarantee success in data acquisition as much as possible, the process we developed to evaluate available commercial systems, and the system we will field.

II. EXPECTED MEASUREMENTS

The measurements we normally make on weapons tests include acceleration, temperature, pressure, radiation, and (more rarely) humidity. We expect to continue to make some of these measurements, but we are also becoming involved in making measurements related to air quality and personnel safety. These include measurements of oxygen, carbon monoxide, and other noxious or potentially explosive gases. We expect to make the measurements in the main tunnel, the alcoves, the refuge stations, and in the experiment drift.

Our major objectives in the main tunnel are to assess damage to the tunnel walls resulting from a test and the conditions in the tunnel before, during, and after a test. The tunnel wall measurements will be made by three triaxial accelerometer arrays on the tunnel roof. Of particular interest will be the differential motion across the main tunnel just after zero time.

In the main tunnel itself we will measure the temperature, pressure, humidity, radiation, and air quality levels. The pressure measurements assume an added importance by the intention to pressurize the main tunnel prior to zero time in order to impede contamination of the main tunnel by porous flow through the alluvium. This measurement will be made at three or four locations in the main tunnel. Subsets of these measurements will be

made in the alcoves and the refuge station, leading to about 90 channels of data.

The containment experiment as proposed by App, et al. 1993 was taken as a reasonable candidate for requiring the greatest number of event specific measurements by our system. These measurements would be with many of the same sensors as in the main tunnel (mostly accelerometers and pressure transducers). The containment experiment would require about 90 channels of data.

Thus, we can expect to collect about 180 channels of data throughout the complex on any given experiment. We do recognize that this is only as guess and that we must be sufficiently flexible to adapt to requirements as they arise.

III. SYSTEM REQUIREMENTS

We first established some requirements that we felt the system must meet based on our expected mode of usage:

1) **Communication** - In order to minimize personnel time underground, we must be able to monitor and control the downhole hardware from above ground. This will be done from the Monitor Room at the CP using either Ethernet or Fiber Data Distributed Interface (FDDI) over the Los Alamos fiber optic backbone. Thus, the downhole system must be able to utilize at least one of these transmission protocols.

2) **Data Transmission** - There is some potential vulnerability from ground shock to the underground hardware. Since our measurements have their greatest value in the event of a problem, it is essential that we obtain data. We therefore decided that we must unfailingly transmit some ground shock data from close to the source uphole before ground shock arrival at the alcove. This would allow us to have some information about ground shock strength in the unlikely event that ground shock disabled our equipment in the alcove. This means that the system must be able to obtain, process, and transmit data on a timely basis.

3) **Redundancy** - We will utilized two completely independent systems to gather data downhole to limit failure to collect any data. Both systems must be on the communication network with the Monitor Room.

4) **Multiple Use** - There may be times when both people uphole and downhole need access to one of the downhole systems. This is most likely when there are problems which may require staff to diagnose and support staff to implement changes. Thus, we require the system to allow multiple users.

5) **Ease of Use** - We do not know how often we will be using the LYNER complex. The software must be sufficiently easy to use that the technicians do not require a long start up time to remember how the system works.

6) **Flexibility** - The many uses to which LYNER may be put are uncertain. We must therefore be ready to respond to any change in direction or new use for the complex. This requires a flexible downhole system. In order to maximize our potential for hardware improvements without being tied to a specific vendor, we would like to have a standard bus (VME or VXI).

7) **Vendor Support** - If we have trouble with the system, there must be a swift response from the vendor support and maintenance people. We believe that this can be most easily accomplished if the digitizing system (both hardware and software) are supplied by one vendor.

8) **Scan Rate Switching** - We must make both dynamic measurements shortly after zero time and longer term environmental measurements (both before and after zero time). These two kinds of measurements require different sampling rates. Thus, we must be able to switch sampling rates easily and automatically.

9) **Data Display** - We will want to display some late time data in the Monitor Room continually in almost real time while processing of the event data is taking place. The plotting display must be easily intelligible and we must be able to see the data accumulate on the screen.

With these criteria we began the process of examining vendors for a downhole instrumentation system.

IV. SYSTEM SELECTION PROCESS

A number of vendors were contacted either directly or by visits to real time data acquisition shows. For those whose system had no obvious reason why it could not meet our needs, we arranged to have the vendor demonstrate the system. We returned to those vendors who we felt might be able to perform our tasks and asked them to perform another demonstration, this time to our specifications. Our test had the following objectives:

Hardware Objectives:
- switching scanning rates and scan lists automatically
- finding restrictions on scanning rate and implementation
- storing data
- receiving external trigger
- exception triggering
- demonstrating alarm condition
- finding channel noise floor
- examining channel crosstalk

Software Objectives
- clarity of use
- data display
- software trigger
- setup steps
- data transmission over Ethernet/FDDI

Personnel Objectives
- knowledge of their system
- cleverness in utilizing system

Four different systems were put through this demonstration. Of these, we have selected a VXI system as the most suitable one for our specific requirement.

V. SYSTEM DETAILS

Each of the two downhole systems includes a downhole controller which is a computer that takes up the first two slots on the VXI bus. The computer includes an Ethernet link. Next in the VXI chassis come several 64 channel analog to digital (A/D) boards, each of which contains its own memory and logic for switching scanning rates. The boards also contain a much smaller memory storage section where the current value of the data for each channel is stored (called the current value table). This can be sampled at arbitrary intervals without interrupting data collection. The aggregate for each A/D board is 100 ksamples per second. If we ever have a situation for which higher sampling rates are required, we have a few two channel, 20 megasamples per second aggregate A/D boards from the same vendor. Also included, but not as part of the VXI bus, are a rack mounted hard disk for data storage and programmable filters and signal conditioners.

The data acquisition will proceed as follows. Prior to zero time we will be recording measurements from sensors throughout the LYNER complex. These data will be extracted from the current value table at about one sample per second except for the accelerometers, which will be sampled at about 100 samples per second. When the first of either a fiducial signal or data trigger has been received, the data is stored in memory at the appropriate sampling rate for each sensor. As these data are collected, they are simultaneously stored on the local hard disk and a few selected datasets transmitted over the Ethernet/FDDI link to the workstation in the Monitor Room. Once a predetermined time (or some other preselected) rate, all the data on the hard disk are transmitted uphole for approximately real time display.

Once the dynamic data has been transferred uphole, a limited amount of automated data processing and graphical display will be performed. When the test is determined to be over, the Ethernet link between the Monitor

Room and Los Alamos will be reestablished and the data transmitted to Los Alamos for final analysis and distribution to our customers. Also transmitted will be previously uploaded signal conditioner gains and filter characteristics.

We believe that the approach we have outlined will ensure successful data acquisition to a great extent. While no system is foolproof, we expect a very high rate of data return from this system even if unexpected events occur.

THREE-DIMENSIONAL POROUS FLOW CALCULATIONS OF THE LYNER CONCEPT

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ABSTRACT

The nature of an underground testing facility for multiple-use, low-yield underground explosions is inherently three-dimensional. The boundary conditions from the blast and those from the main access drift are essentially orthogonal. To address the containment-related problems and possible solutions of the LYNER facility, an extensive series of 3-D porous flow calculations was performed. First, access drift and room/cavity dimensions were determined. Next, specific containment features, such as massive grout plugs, were incorporated into the analysis. Material properties for all geologic and containment related materials were then established. Finally, realistic pressure and tracer (radiation) boundary conditions were applied to the cavity walls. The resulting time-dependent distributions of gas-borne contamination established a baseline case. Then, several different remedial actions were investigated numerically, including time-dependent pressurization of the access drift and the emplacement of low permeability (but still leaky) doors to maintain a pressure gradient away from the "clean" access regions. The utility of such a computational capability has been to provide (at a very low cost) an "order-of-magnitude" estimate of compressed air requirements and "stand-off" distances to assure that the underground test facility can be operated with minimal risk of contamination of the entire complex. By using a porous flow code that had been tested and validated for containment-related problems (TRACR3D), the results are reasonably believable, subject to the assumptions and limitations of such an analysis. Also, the use of state-of-the-art, three-dimensional graphics capabilities, provided a unique opportunity to "see into" the problem and numerically investigate novel solutions, based on these insights which are unavailable in a two-dimensional environment.

INTRODUCTION

The LYNER concept is inherently three-dimensional. The boundary conditions from the blast and those from the main access drift are essentially orthogonal, with pressures which are equivalent within spatial and temporal regions of interest. While many of the qualitative gas-flow aspects of the proposed underground testing plan may be addressed using superposition of two-dimensional calculations, detailed three-dimensional simulations will have to be made at some point to provide numerical confirmation of the assumptions. Moreover, it is useful to perform a few 3-D calculations to gain a perspective of how the different driving processes interact and to visualize the relative dimensions of the different features and how they change with time, especially the pressure and contamination plumes.

This is the first time three-dimensional porous flow calculations have ever been applied to any containment scenario. Typically, tamped vertical shots are easily simplified with two-dimensional cylindrical symmetry. As with any new calculational approach, the technique evolves at nearly the same pace as the insight gained from the output. As a result, there are many calculations that appear, from hindsight, to have been ill-conceived. The 3-D calculations tend to be quite resource-intensive, with most of the runs described here running for ten or more hours of computer time (single-processor Cray YMP). In defense of the code, at least half of the time spent was performing costly formatted output. It must be emphasized that the extension from two to three dimensions requires a huge increase in the analysis required to understand what has been calculated, to make meaningful changes, and to present the results in understandable illustrations. The state-of-the-art in processing three-dimensional data is progressing at an astonishing rate, as shown by some of the graphics in this paper. Each output edit will print tens-of-thousands of numbers, such as pressure and tracer (radiation) concentration for each of the calculational cells. Decisions must be made at the beginning of a simulation as to how frequently the output data shall be printed. Not too often, so as to bog down the physics, but often enough so that days or months after the calculation has been completed, one can go back and obtain required information. Since the post-processing algorithms for three-dimensional graphics are being

developed at the present time, the amount of computer time can be expected to decrease in the future, as more efficient ways of writing and storing data are implemented with time. Also, we will be much smarter with respect to this particular three-dimensional problem, in terms of anticipated material properties and boundary conditions. As with any modeling project, perhaps 90 percent of the effort is required in setting up the first realistic problem, and 90 percent of what is ultimately presented comes with only 10 percent of additional effort. Hopefully, we have performed that most difficult task of setting up and running these first (realistic?) simulations.

SETTING UP THE PROBLEM

The main disadvantages of three-dimensional simulations are, therefore, the large amount of computing time required for the analysis and the difficulty of sifting through the enormous amount of output to understand what has been calculated. As it turns out, quite a lot can be learned from any 3-D simulation, even if it turns out to be ill-conceived (silly) in retrospect. The evolution of the eleven calculations performed for this project will be presented, and four will be described in detail.

The objective of the calculations was to determine the effect of an underground blast on a relatively nearby access drift which would be a permanent feature. This drift must not be contaminated by porous flow of explosive gases generated from the detonation and driven by the high pressures generated in the cavity, for tamped shots, or the zero room, for untamped events. It is believed contamination might be mitigated by pressurizing the drift prior to and/or during the shot. It was intended that the calculations could help quantify the flow requirements so that Field Engineering (J-6) could make an evaluation as to whether or not the concept was economically feasible.

The initial calculation was setup as shown in Fig. 1. There are two planes of symmetry and the dimensions are 40 x 40 x 20 m deep. The cavity is assumed to be 4 x 4 x 4 m or 64 cubic meters. There are two grout plugs, each with a cross-section of 16 square meters; one adjacent to the cavity 8 m long and another 6 m long, separated by a 2 m section of high permeability

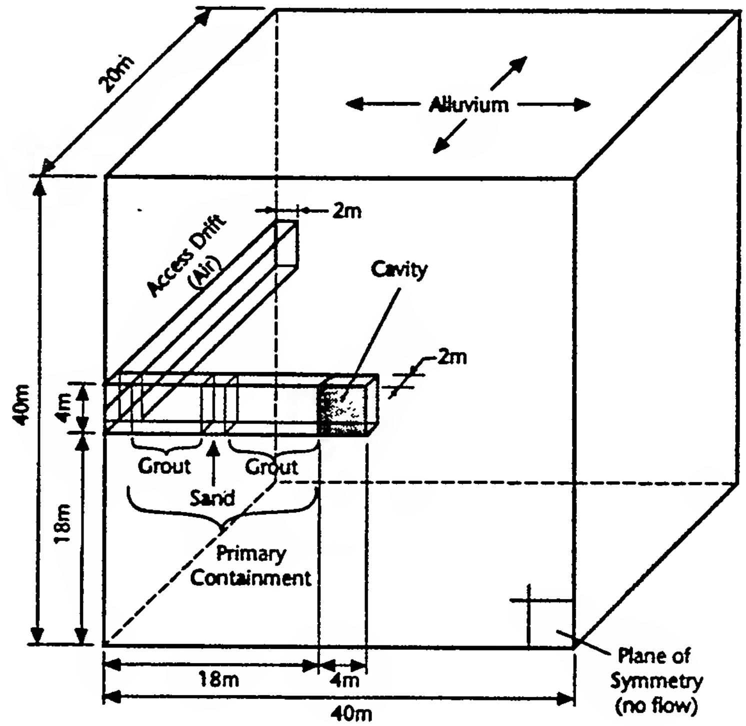


Figure 1. Initial configuration for the three-dimensional porous flow calculations. Here there are two planes of symmetry. Notice the two grout plugs, separated by a high permeability region. The center of the cavity is 20m from the center of the access drift. The zoning within this region is 2 x 2 x 2m.

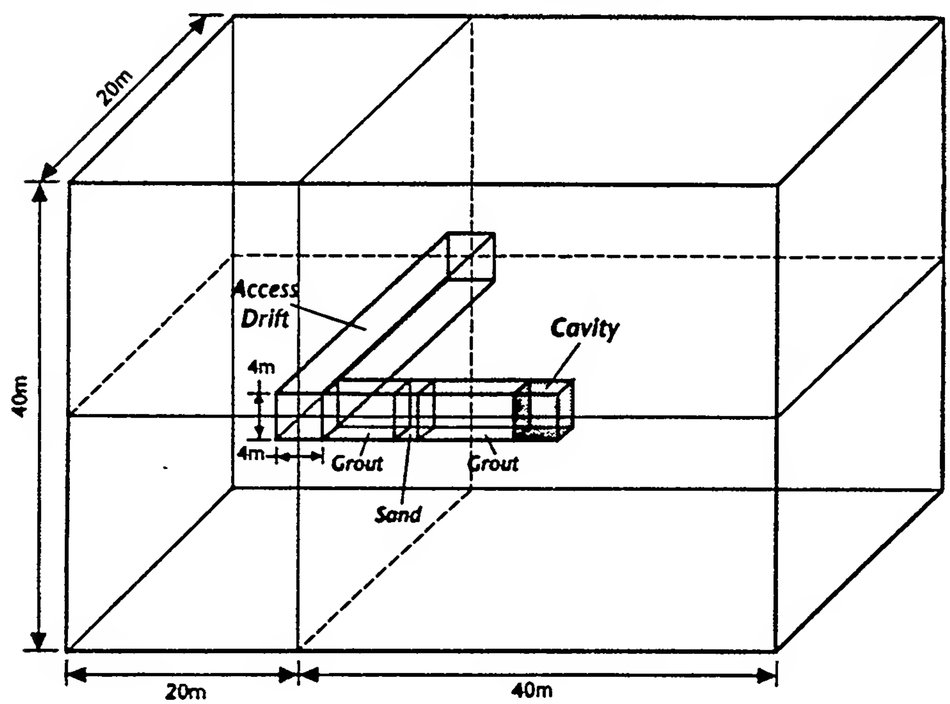


Figure 2. Modified mesh with only one plane of symmetry (through the cavity). Notice that there is a horizontal plane of symmetry through the cavity and the tunnel. This was not taken advantage of, since the permeabilities of the alluvlium, generally decrease with depth.

material. The access drift is assumed to have a cross-section of 16 square meters and is open at the back of the mesh.

The material properties varied somewhat during the calculations. To avoid confusion, Table 1 provides the final material properties (Mods 9 and 10).

Table 1. Final Material Properties for TRACR3D 3-D Calculations of LYNER

Material	Permeability	Porosity	Constrictivity
Alluvium	1.44 darcy	0.15	0.3
Drift	50	0.95	0.3
Grout	0.1	0.02	0.1
Sand	1000	0.40	0.3
Melt (Compact)	0.15	0.10	0.2
Cavity/Room	50	0.95	0.3

There are many more material properties required for input in the TRACR3D code, but they were either identical for each material, or they were not utilized due to the simplifications made, e.g. only gas flow was allowed (no water flow) and all materials were dry. As described below, the permeability in the cavity and drift was initially much larger (1000 d) but had to be lowered for time-step considerations.

SUMMARY OF CALCULATIONS PERFORMED

The following section briefly describes the calculations performed, expected results and what actually happened. It should be noted that the contamination is represented by the code as the concentration of a tracer with properties of a specific species, in this case xenon-133 with a half-life of 2.3 days. This is not exactly equivalent to radiation dose as measured in R/hr, but is assumed to be roughly comparable in terms of the order-of-magnitude decrease from the most contaminated region (the cavity). For this reason, six orders of magnitude were assumed to be the detectable limits, since contamination measurements recorded in containment contexts have

typically varied from 0.01 to 10000 R/hr, or six orders of magnitude. It is suggested that a more thorough analysis of this assumption be made in the future.

The distance separating the cavity (or zero room) from the drift was chosen (and approved by consensus) to be 20 m. It soon became clear that this distance was probably too small based on ground shock and hydrofracture considerations, but the 3-D calculations were well under way and could not be easily modified. The distance surely provides a "worst-case" scenario, and the effects of drift pressurization are much more noticeable under these conditions. As the field implementation takes place, additional simulations will be required to model the actual design.

The best way to view these simulations is to imagine the enormous resources required to actually conduct the different field tests described below. Clearly, a field test gives us more confidence, since it is conducted in real materials. On the other hand, a calculation provides complete knowledge of the pressure and contamination fields, which is not possible under actual field conditions. At this point, any calculation will simply provide a qualitative sense of tradeoffs, that should only be used to supplement sound engineering judgement.

Mod 0 - Using the geometry shown in Fig. 1, the cavity was pressurized to 20 bars (gauge pressure) and allowed to decay into the formation. The permeability of the alluvium was uniform with a value of 7.44 darcy. **Result** - The first output edit was at two hours (problem time), and the high permeability had the effect of dissipating all the pressure by this time. Also an infinite half-life was used, so the residual contamination extending out 3m beyond the cavity did not dissipate after 20 hours.

Mod1 - Two changes were made - first the alluvium permeability was dropped to 1.44 d (more consistent with measurements made at the Ledoux depth) and the pressure boundary condition was modified to represent an actual cavity pressure measurement at a depth and yield roughly equivalent to what LYNER would be expected to accommodate. **Result** - The larger

pressure caused cavity gas gradients to rapidly reach the limits of the calculational grid. Unfortunately, the timestep was significantly limited due to the large difference in material permeabilities and the pressure gradients. The extent of contamination extended to 11 m after only 64 minutes.

Mod 2 - The permeability of the drift and the cavity region was reduced to only 10 darcy to help with the timestep. **Result** - The calculation proceeded out to two hours and the influence of the grout plugs was shown to be effective in inhibiting the flow towards the access drift. It was clear that the boundaries of the problem were far too close to the cavity, as pressure contours were stacking up along the edges of the grid, where the pressures were forced to ambient (0.9 bar). Also, it was realized that there was no plane of symmetry through the access drift, unless, of course, there is an identical explosion on the opposite side of the drift, with identical characteristics. This would have the effect of causing a plane of stagnation along the drift which is a poor analog for what is expected to take place in the field.

Mod 3 - Two major modifications were made. First, the plane of symmetry along the drift was removed, as shown in Fig. 2. This required 50 percent more cells as the total volume modeled increased from 32000 to 48000 cubic meters. Also, a region of increasingly larger cells was wrapped around the simulation region of interest (high resolution). There were now 30 x 10 x 20 = 6000 high resolution (2 x 2 x 2 meter) cells, and the overall mesh size was 38 x 14 x 28 = 14896 total cells, and the total simulation volume increased to over 1 million cubic meters (2 million from symmetry). Two views of the large mesh are shown in Figs. 3a and 3b. **Result** - The new mesh seemed to work well, the results were essentially an extension of Mod 2, since no material properties were changed. It was clear that the pressure arrivals were too early, i.e., not consistent with pressure measurements in RAMS packages typically fielded on vertically emplaced events.

Mod 4 - A solution for this which has been implemented in the past [1,2] has been to introduce a low permeability, compacted region around the cavity, to simulate the phenomenology of melt and compaction (decreases in permeability and porosity). This was accomplished here by surrounding the

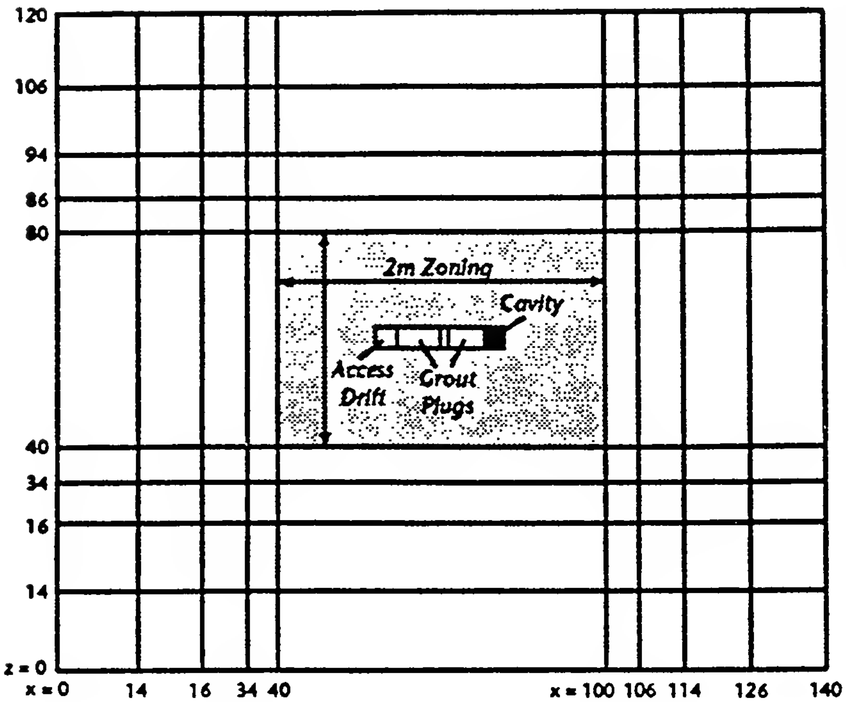


Figure 3a.
A vertical slice through the extended mesh showing the large cells which were used to allow gas to flow away from the region of interest. The shaded area represents the finely zoned mesh shown in Fig. 2.

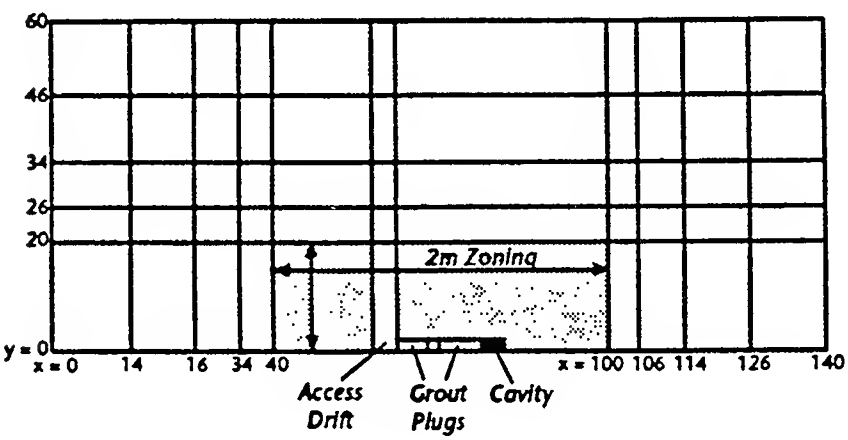
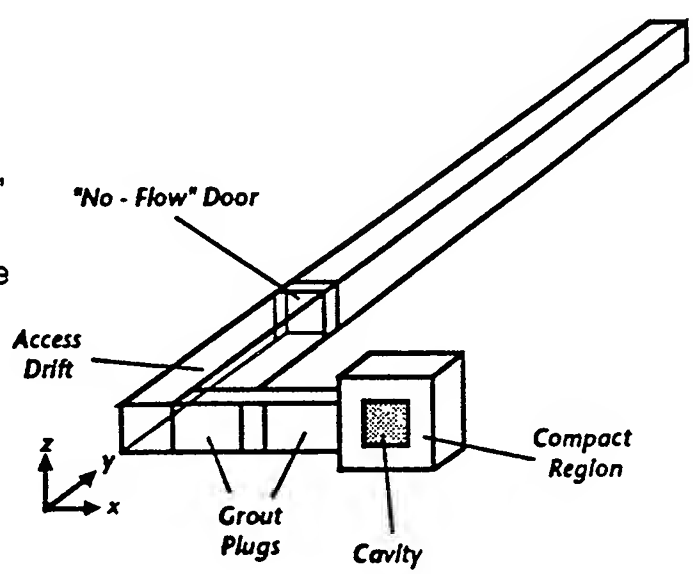


Figure 3b.
A plan view of the extended mesh showing the containment features, the large zones and the fine mesh shown in Fig. 2.

Figure 4. A schematic view of the cavity, the compact (melt) region, the primary containment (grout plugs), the access drift and the general location of the "no-flow" door.



cavity with cells with these characteristics. The compact region from previous (two-dimensional, i.e. cheap) simulations was determined to be only about one tenth of the cavity diameter, and the permeability reduction from the geologic media (after several trials) was a factor of 50, or more. Here, our smallest cell size is one-half of the cavity diameter, so the permeability of the compact region was set to 0.24 darcy instead of 0.05 darcy [1,2]. The compact region is shown schematically in Fig. 4. **Result** - This material property change substantially reduced the flow of gas from the cavity and the transport of the tracer. The calculation was run for 15 minutes and the grout plugs were effective at slowing down the transport of contamination. Even so, the distance to the access drift (20 m) was reached by this time.

Mod 5 - The compact region permeability was decreased to 0.15 darcy, which was still greater than the grout plugs, but smaller than all other materials. This calculation is the baseline tamped three-dimensional simulation run with no drift pressurization and was setup to run for one day. **Result** - The calculation actually ran for nearly 16 hours of simulated time. It was clear that significant amounts of contamination would reach the access drift within these times, although the pressures at the drift walls were always less than a few hundred millibars above ambient, leading to the hope that drift pressurization could keep contamination away. This calculation is described in detail in a later section.

Mod 6 - The drift was pressurized by specifying the mass of air injected in certain cells as a function of time. The cells selected were the first layer of cells in the drift, i.e. those adjacent to the plane of symmetry. A "no-flow" boundary was set up in the drift at a distance of 20m in order to build up pressure. Note that gas could still flow through the alluvium to the other side of the drift and this barrier simply represents an impediment to flow through the drift and does not imply a massive shock-resistant structure. The magnitude of gas injection was

(235.97 g/sec/cell) x (4 cells [x 2 by sym.]) x 1000 cfm / (471.9 g/s) = 4000 cfm.

The gas injection rate dropped to 2000 cfm after one hour and then to 500 cfm after two hours. A change was made to the pressure boundary condition to allow simple decay after approximately one hour (when the cavity pressure data is unavailable due to collapse of the measured cavity) rather than forcing the pressure to remain at the last value (1.9 bars). **Result** - At early times, the pressure field was dominated by the cavity gases flowing out into the formation. After 30 minutes the pressures were roughly equivalent in the drift and the cavity, but significant flow had already contaminated the drift by this time, so that even though the pressure gradients were all directed away from the drift after one hour, quite a lot of contamination remained even after nearly 5 hours, especially downstream from the impermeable doors. This calculation is described in detail in a later section.

Mod 7 - The next step was to pre-pressurize the drift, as an initial condition, so that pressure would be available to push against the pressure wave coming in from the cavity. A pressure in the drift of 1.2 bars was established and the impermeable door was moved back 14m to keep contaminated gas from moving into the unpressurized portion of the drift. Also, the magnitude of gas injection was decreased to 50 cfm after two hours since it is the only source of pressure at that time and only a small amount of gas should be required to keep contaminated gas away as decay takes place. **Result** - There was some effect, but it was very minor. Although the drift was pressurized (instantaneously) the alluvium was still at ambient conditions (0.9 bar) so the mass of pre-pressurized air was quite small compared to what was coming out of the cavity.

Mod 8 - It was then decided to pre-pressurize the drift for two hours prior to detonation, so as to allow the alluvium to become pressurized and act against the pressure wave from the cavity. Also, a second source of pressure was added in the drift (or four by symmetry) at a location 20m from the plane of symmetry (14m from the "no flow" door in the drift). Also it was discovered that an input error had been made in the gas injection time histories, so that Mod 6 was injecting 1000 cfm for the entire time and Mod 7 was injecting 100 cfm. The injection rate was set to 8000 cfm for 4 hours [500 cfm/cell x 4 cells/injection point x 2 injection points x 2 (sym)=8000] (two hours prior to the detonation and two after) decreasing to 4000 cfm for two additional

hours, then dropping to 100 cfm. **Result** - Unfortunately, the code was not able to accommodate changing the required parameters upon restart, so the calculation simply pressurized from the drift and never saw the explosion from the cavity.

Mod 9 - The cavity and drift time histories were "hard-wired" into the input of the code at start time, so a restart was required only to drop the time-step associated with the explosion in the cavity. This is less favorable, since the pressurization must always be performed first, even though nothing has changed during this phase. The TRACR3D code is being modified to allow more significant modifications at restart time to accommodate these kinds of problems. Another bug was found in the input prior to the start of this problem, in that the time histories always interpolate between values, rather than maintaining the previous value until the next time in the history is encountered. The input was modified to correctly provide the injection rates specified in Mod 8. Prior to actually submitting the run, the injection seemed to be more than sufficient, so the total injection rate was modified so that 250 cfm per cell for a total of 4000 cfm was injected for four hours (2 prior to detonation and 2 after detonation) and then the rate dropped to 50 cfm per cell (800 cfm total). Also the location of the second injection point was moved 4m closer to the plane of symmetry (away from the "no flow" door) to sustain the pressure in the mid-section of the drift. **Result** - The pre-pressurization was sufficient to keep all detectable (six-orders of magnitude) amounts of contamination away from the drift for the four hours of simulated time. The pressure in the drift after this time seems sufficient to keep the contamination at bay while it decays away. This is the final calculation for the tamped case and is discussed in detail in a later section.

Mod 10 - This is essentially identical to Mod 9, except the pressure from detonation is assumed to be confined to a room having a volume of 576 cubic meters (recall Ledoux was 1100 cubic meters). The dimensions of the room are 4 x 2 (x 2 by symmetry) x 36 m. The room begins 20 m from the drift and is separated by the same grout plugs as initially described. The pressure boundary conditions are 20 bars, which simply decay out into the formation. This represents generation of 20 roomfuls of noncondensable gas, which is allowed to bleed out into the formation, much like the Ledoux

case [3]. The pre-pressurization of the drift and alluvium is identical to the tamped case, described for Mod 9. **Result** - At very early times, the pressure field is governed by the pressure generated from the "zero room" as gas runs away from the room. Later, the pressure generated from the drift is sufficient to turn the gradient away from the drift and keep all the contamination away from the drift. In fact, since more pressure and contamination enters the alluvium at distances greater than Mod 9 (since the source is much longer), there seems to be less opportunity for drift contamination in this case (Mod 10). Details of this case are presented later.

The simulations presented in this section provide a great detail of qualitative information regarding the importance of making certain changes in the assumed material properties and pressure boundary conditions of the drift and the cavity (or zero room). The quantitative values cited, such as the injection rates, must be regarded as preliminary, as they will depend greatly upon all the assumptions inherent to the calculations. For example, we have only assumed a single value of permeability for the alluvium, although it is well known from the Ledoux experience that a wide variety of permeabilities exist. The utility of a numerical approach to simulate actual underground conditions, especially for strongly three-dimensional problems like this, is enormous. Having demonstrated the effect of certain parameters, it is now necessary to design an underground system and establish material properties and boundary conditions (such the locations of any "no-flow" or "low-flow" doors) that are likely to represent the actual environment. Then, new three-dimensional calculations can be performed to provide increasing confidence in the quantitative values generated by the code.

DETAILS OF SPECIFIC CALCULATIONS

Four calculations were selected to show output. It is important to emphasize that all numerical values shown in this section are preliminary and they can be expected to change as the design of the actual underground complex evolves and material properties become characterized.

Simple Detonation with No Drift Pressurization

Mod 5 described above, was essentially a one-dimensional calculation since the complicated geometries inherent to LYNER were not active. By this calculation, the material properties have all been specified and only minor changes were made in later runs to the cavity pressure history. Contour plots were generated at three z-elevations (giving plan views). Plots were made of pressure, tracer concentration and velocity above and below the working point and through the cavity and drift complex. The velocity vector plots in this appendix are for an elevation just above the cavity and drift, because the view through the working point elevation is dominated by the high flow in the drift, normal to the plane of symmetry. The pressure and concentration contours, however, are shown at an elevation through the working point. The locations of the containment features are shown in Fig. 2 (and Fig. 12b).

Figure 5a shows pressure contours after 5 minutes of simulated time. The center of the cavity is at x=80m and the drift intersects the x-axis at x=60 m. The pressure in the cavity is about 7.5 bars at this time, but contours are only plotted to 1.9 bars. Recall ambient pressure is 0.9 bars everywhere (no gravity). The 1.0 bar contour has nearly reached the drift wall. Figure 5b shows the corresponding tracer concentration at this time. The contours are slightly exaggerated beyond y=20 m, due to the large zone size beyond this point. Along the x-axis, the influence of the grout plugs can be seen in slowing down the tracer arrivals along the plane of symmetry. Unfortunately, the drift is intersected by these same contours, a short distance away, say y=2 m. Figure 5c shows the velocity vectors at this time. Generally, the flow is directed outward from the cavity everywhere. Recall that for the velocity vector plots, the elevation is slightly above the working point, the drift and therefore the grout plugs. At the working point elevation, the velocities are significantly reduced, as reflected in the Figs. 5a and 5b. The lesson learned, even at this very early time, is that the grout plugs need not be infinitely impermeable, since the gas will flow right around them at early times. It is important that the grout plugs not be the most permeable path, but beyond that, porous flow through the alluvium will dominate contamination toward the drift complex.

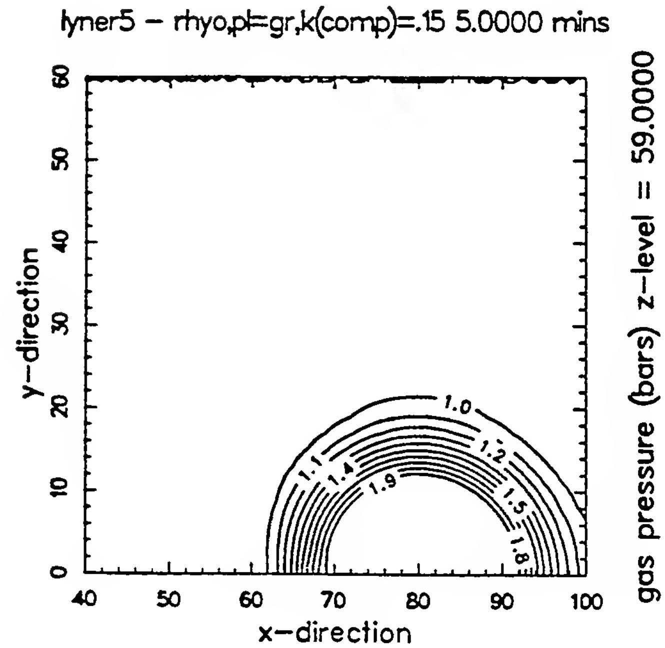


Figure 5a. Pressure contours (to 1.9 bars) at 5 minutes after detonation for the case of no tunnel pressurization (Mod5).

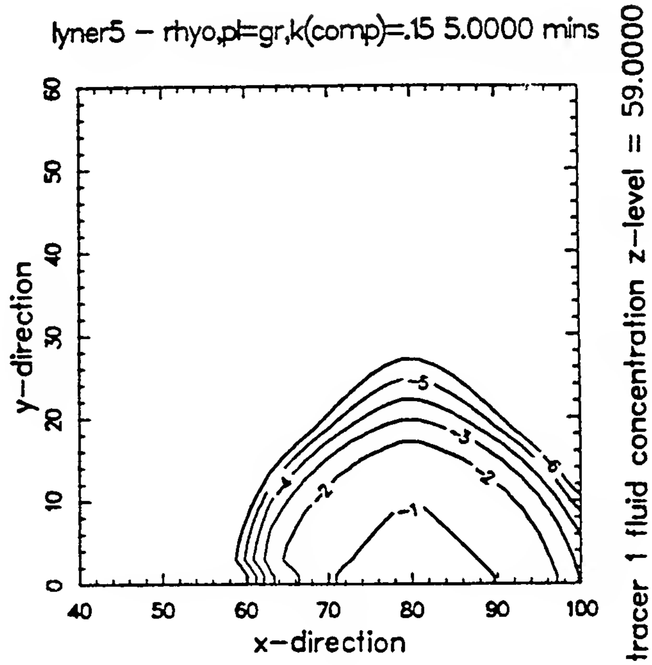


Figure 5b. Contamination contours (order-of-magnitude) at 5 minutes after detonation for the case of no tunnel pressurization (Mod5).

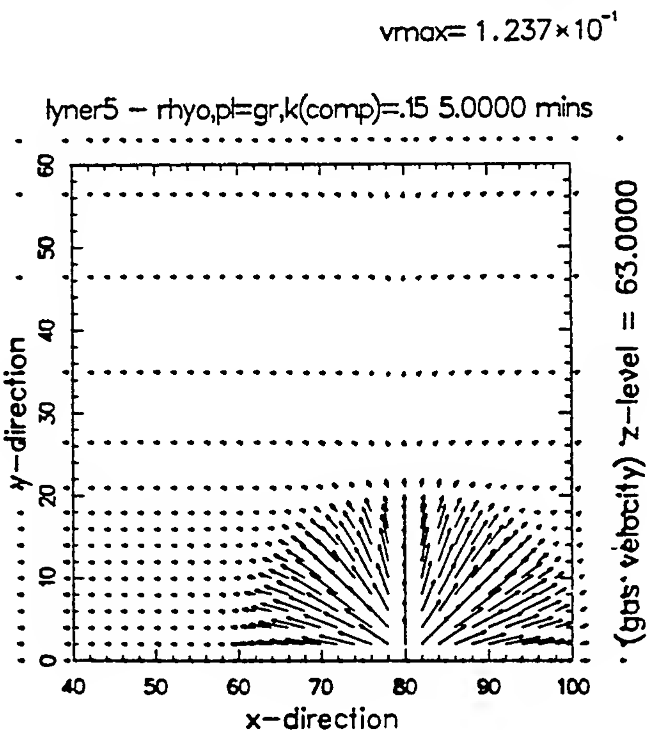


Figure 5c. Velocity vectors at 5 minutes after detonation for the case of no tunnel pressurization (Mod5).

After 15 minutes, the pressure and concentration contours are shown in Figs. 6a and 6b, respectively. The outermost pressure contour is now seen to be influenced by the presence of the (unpressurized, high-permeability) drift. The contamination has clearly reached the drift by this time. The velocity vectors are shown in Fig. 6c. Similar plots are shown in Figs. 7a, 7b and 7c for pressure, tracer concentration and velocity at a time of one hour, respectively.

The only conclusion is that the contamination of the drift by this time is unacceptably great and must be mitigated by moving the source of contamination further away from the drift, or pressurizing the drift with air to act against the pressure from the cavity.

Drift Pressurization Beginning at Shot Time

In order to counteract the pressure wave from the cavity, air was injected into the drift beginning at zero time, i.e. concurrent with cavity pressurization. It was originally planned to inject 4000 cfm into the drift for 1 hour, then dropping to 2000 cfm and finally after 2 hours using a flow rate of 1000 cfm. Due to an error in the input, the higher pressures were only active for a few seconds, so the problem was essentially run at an injection rate of 1000 cfm (including the symmetric portion). This was Mod 6, described above.

Figures 8a to 8c show the pressure, concentration and velocity vectors for this case at five minutes. There is a small perturbation in the pressure field, compared to Fig. 5a, but the concentration and velocity vectors are essentially identical to the unpressurized case at this early time. Similar plots at 15 minutes are shown for this case in Figs. 9a to 9c. By this time the pressure in the drift has become equivalent to the lower contour values generated by the cavity. The drift pressure has significantly modified the concentration contours. The drift, however, has already been contaminated. The velocity vectors also show the influence of drift pressurization.

After one hour, the pressure in the cavity is sufficiently low that it is no longer the driving mechanism in the problem, as shown in Fig. 10a. The

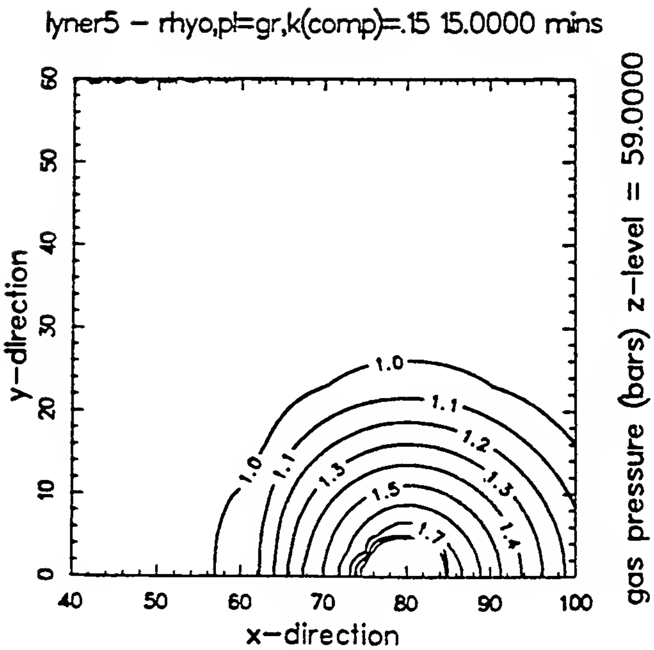


Figure 6a. Pressure contours (to 1.9 bars) at 15 minutes after detonation for the case of no tunnel pressurization (Mod5).

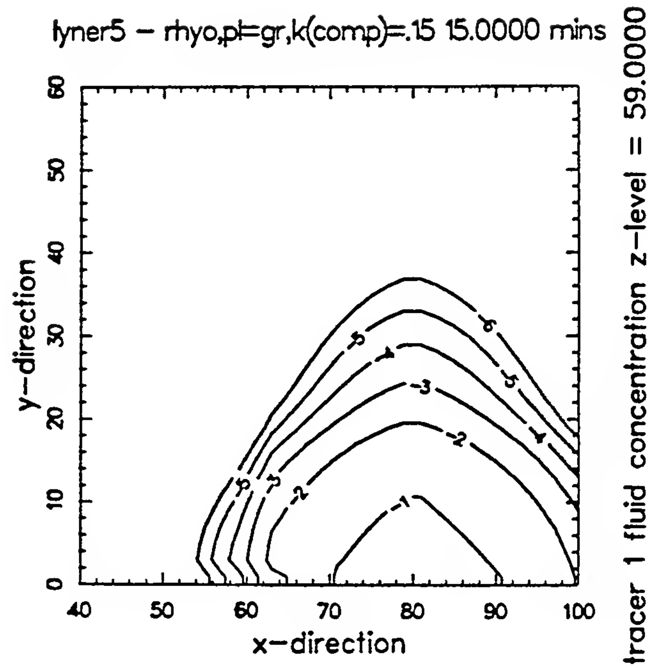


Figure 6b. Contamination contours (order-of-magnitude) at 15 minutes after detonation for the case of no tunnel pressurization (Mod5).

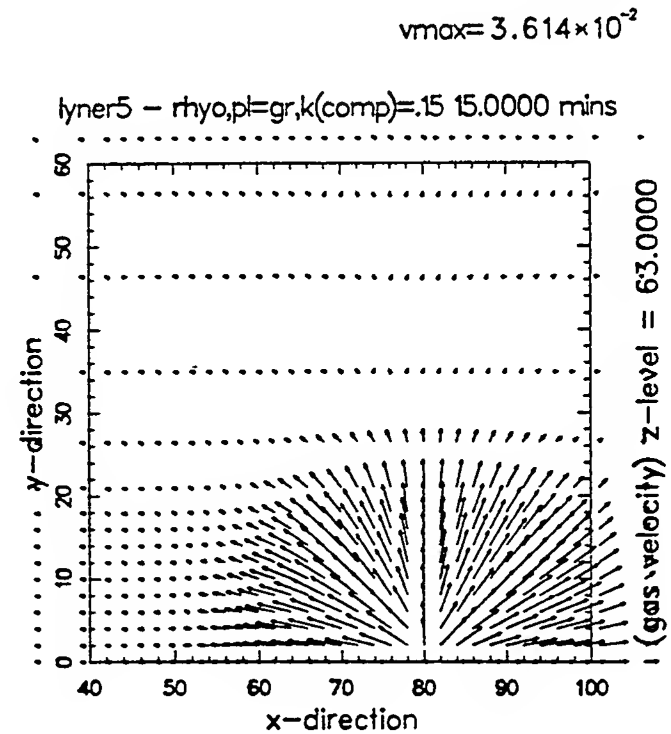


Figure 6c. Velocity vectors at 15 minutes after detonation for the case of no tunnel pressurization (Mod5).

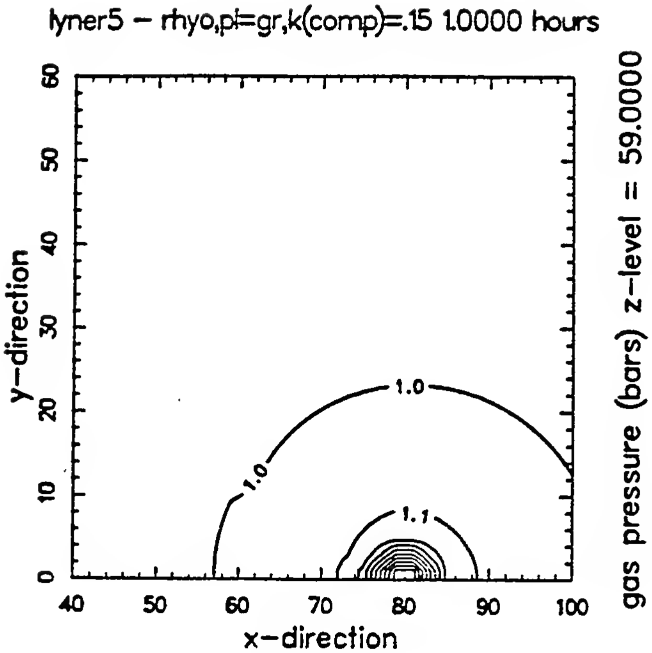


Figure 7a. Pressure contours (to 1.9 bars) at 1 hour after detonation for the case of no tunnel pressurization (Mod5).

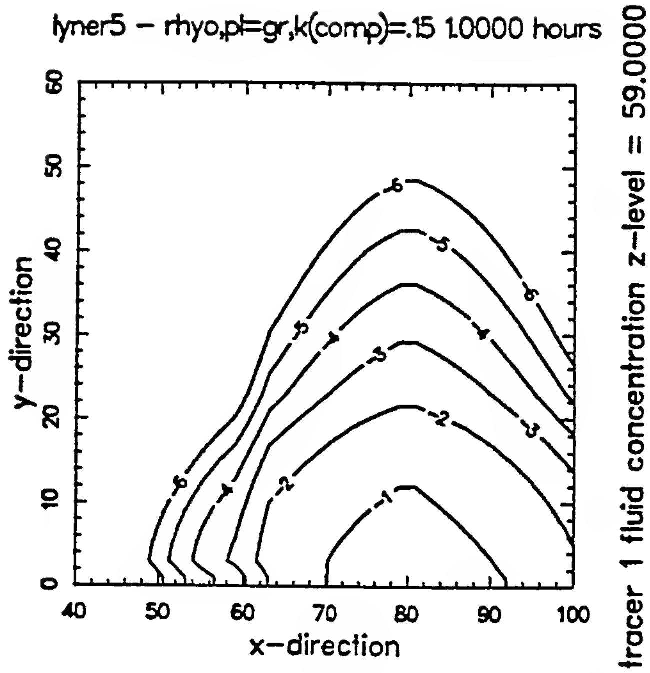


Figure 7b. Contamination contours (order-of-magnitude) at 1 hour after detonation for the case of no tunnel pressurization (Mod5).

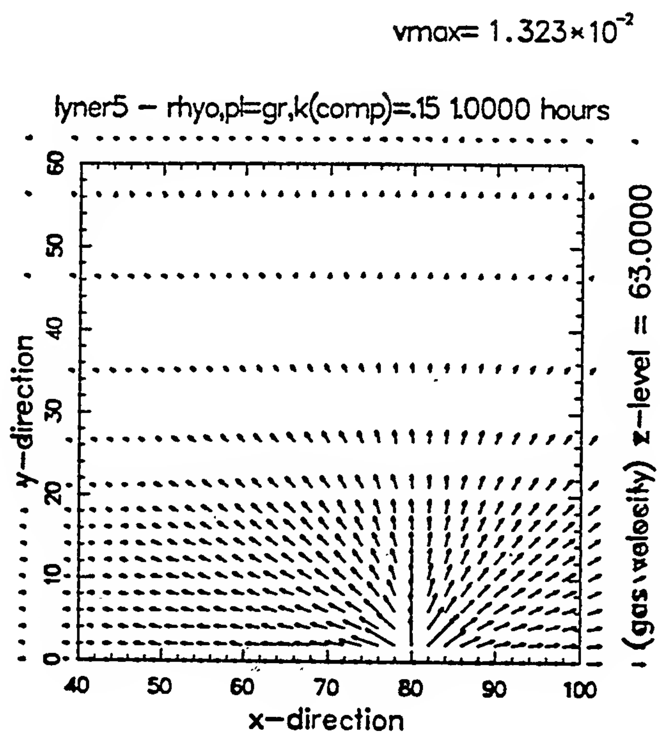


Figure 7c. Velocity vectors at 1 hour after detonation for the case of no tunnel pressurization (Mod5).

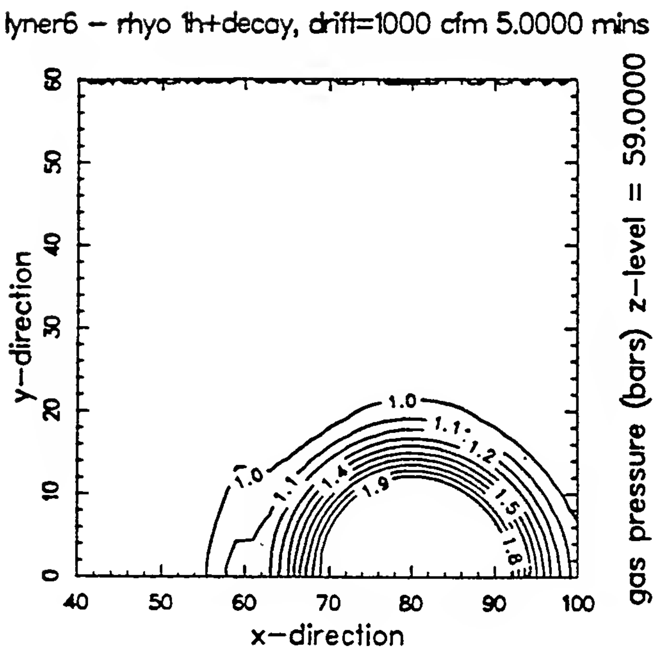


Figure 8a. Pressure contours (to 1.9 bars) at 5 minutes after detonation for the case of tunnel pressurization starting at shot time (Mod6).

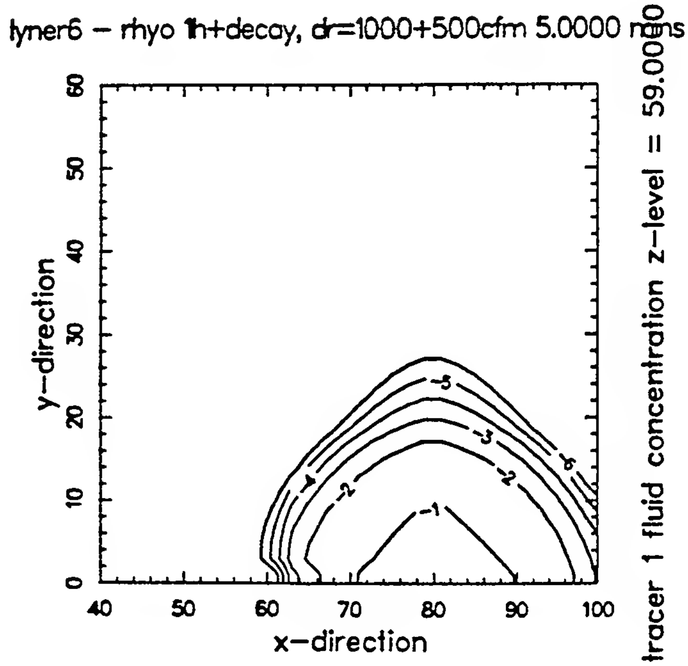


Figure 8b. Contamination contours (order-of-magnitude) at 5 minutes after detonation for the case of tunnel pressurization starting at shot time (Mod6).

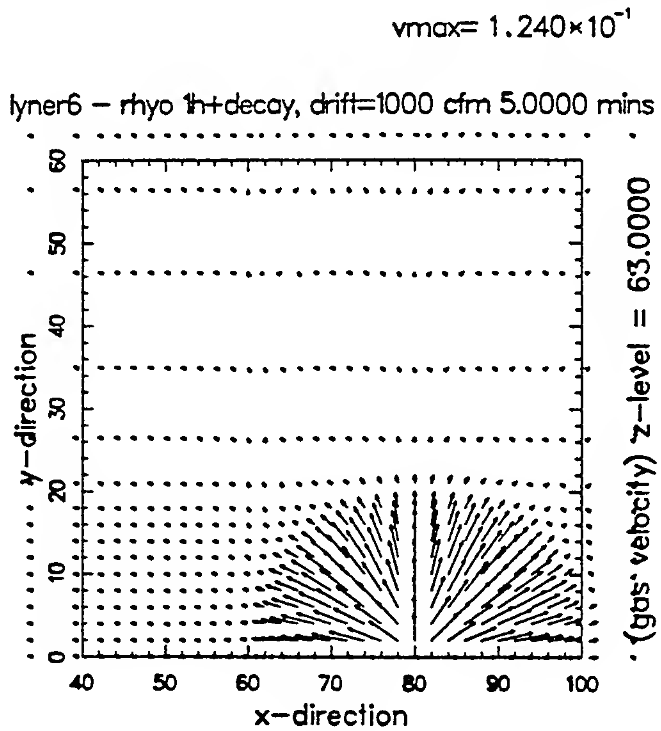


Figure 8c. Velocity vectors at 5 minutes after detonation for the case of tunnel pressurization starting at shot time (Mod6).

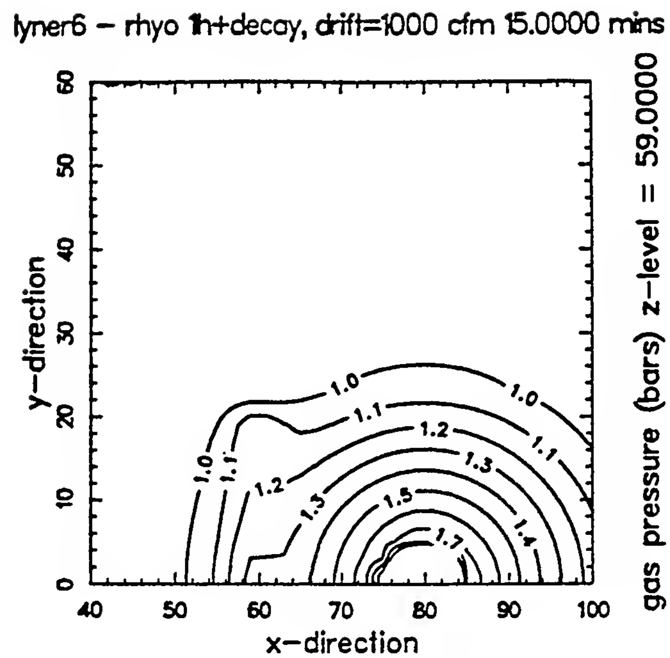


Figure 9a. Pressure contours (to 1.9 bars) at 15 minutes after detonation for the case of tunnel pressurization starting at shot time (Mod6).

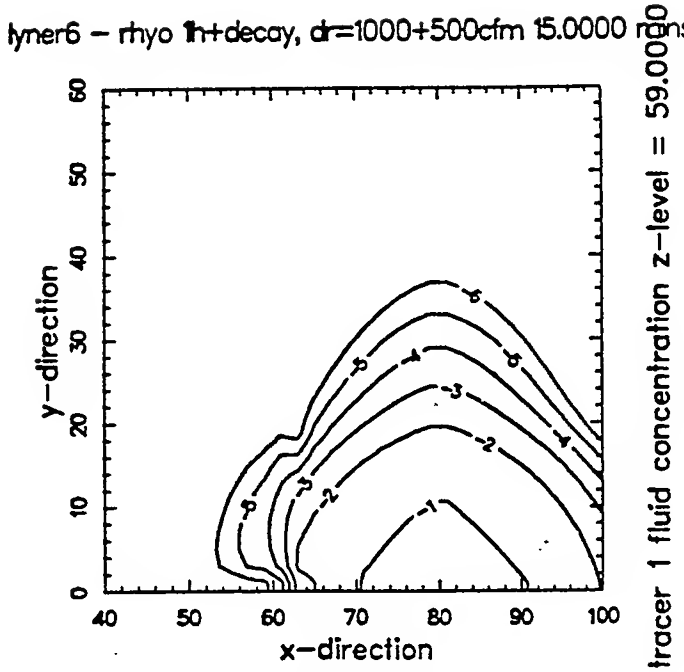


Figure 9b. Contamination contours (order-of-magnitude) at 15 minutes after detonation for the case of tunnel pressurization starting at shot time (Mod6).

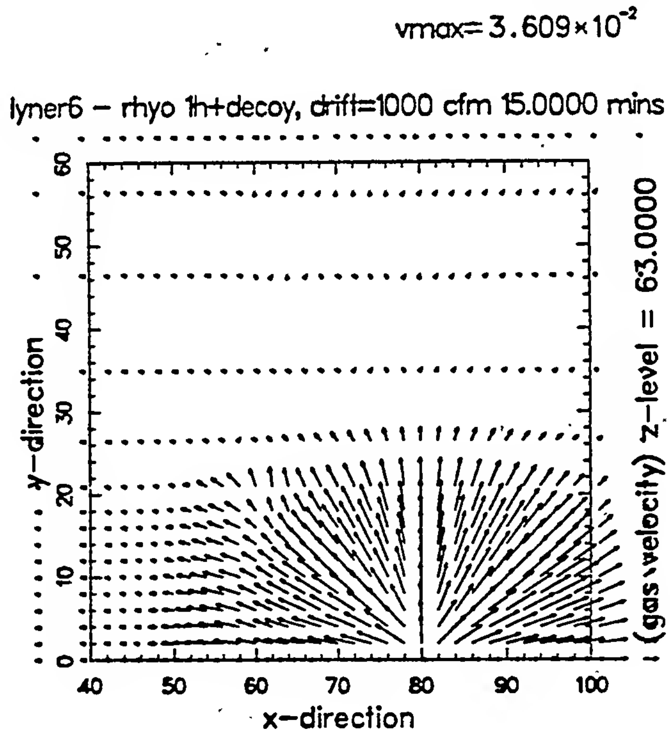


Figure 9c. Velocity vectors at 15 minutes after detonation for the case of tunnel pressurization starting at shot time (Mod6).

location of the "no-flow" door in the drift at 20 meters is clearly evident in this figure. Contamination that has passed through the drift is now pushed away in all directions by the pressure generated within the drift as shown in Fig. 10b. The velocity vectors in Fig. 10c show the relative importance of the drift and the cavity at this late time. The situation after four hours is shown in Figs. 11a to 11c. The drift has reached steady state with the alluvium and the impermeable door is clearly evident. The ability of the pressurized gas to flow around the door is obvious from this plot. The contamination contours are somewhat more complex by this time, as the pressure in the drift has acted to "clean out" the pressurized portion, at the expense of contaminating the unpressurized part of the drift and the alluvium to the left of the drift. The velocity vectors also indicate that cavity pressure is no longer a driving force at this late time.

Even though the amount of gas injected was only one-fourth of that intended, two features are clear. First, the situation is significantly better than the previous case, in that the pressures from the drift have been shown to alter the contamination field. The 1000 cfm is a relatively modest injection rate which could easily be accommodated. It is also clear that the injection was not sufficient to avoid contamination of the drift at early times. We then decided that if pressurization equipment were to be available at zero time, then it could surely be turned on a few hours before shot time, so as to "pre-stress" the pore space in the alluvium, thereby allowing a more effective curtain acting against the pressure and contamination generated by the cavity.

<u>Drift Pressurization Two Hours Prior to Shot Time</u>

This is the baseline tamped case that will be presented. It was described above as Mod 9. There are two injection points, one at the plane of symmetry, y=0, and one at a distance of y=16 m. The doors of the drift are at y=34 m. Each cell was injecting 250 cfm and there were four cells per injection point, resulting in a total flow of 2000 cfm. The total simulated flow was therefore 4000 cfm due to the plane of symmetry. The simulated drift length was 2 x 34 = 68 m. Figure 12a shows pressure contours at a time of 2 hours after injection. Over one atmosphere of overpressure is present in

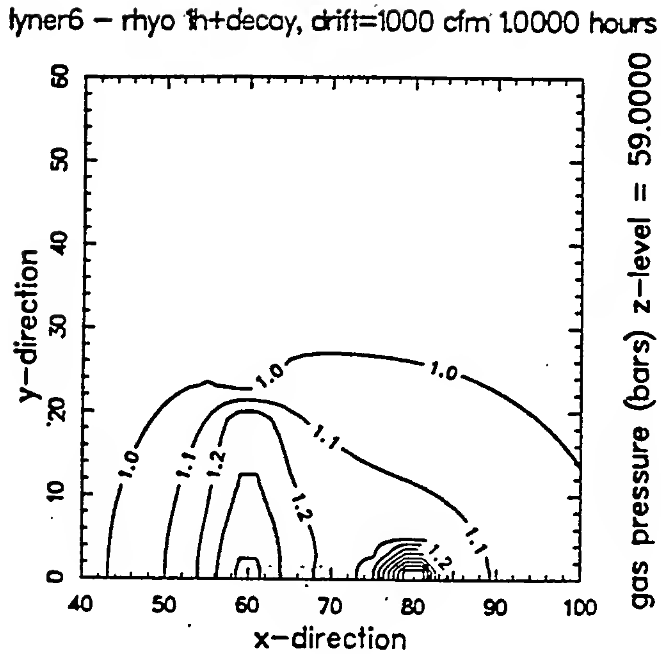


Figure 10a. Pressure contours (to 1.9 bars) at 1 hour after detonation for the case of tunnel pressurization starting at shot time (Mod6).

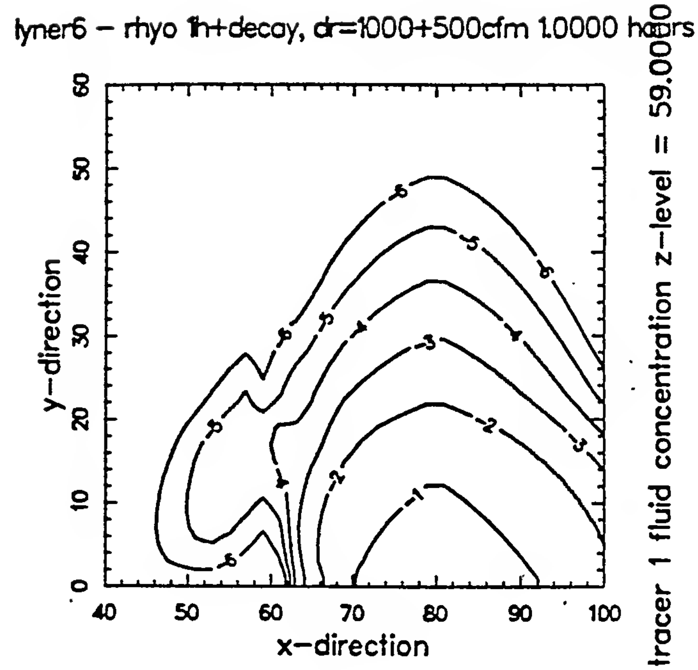


Figure 10b. Contamination contours (order-of-magnitude) at 1 hour after detonation for the case of tunnel pressurization starting at shot time (Mod6).

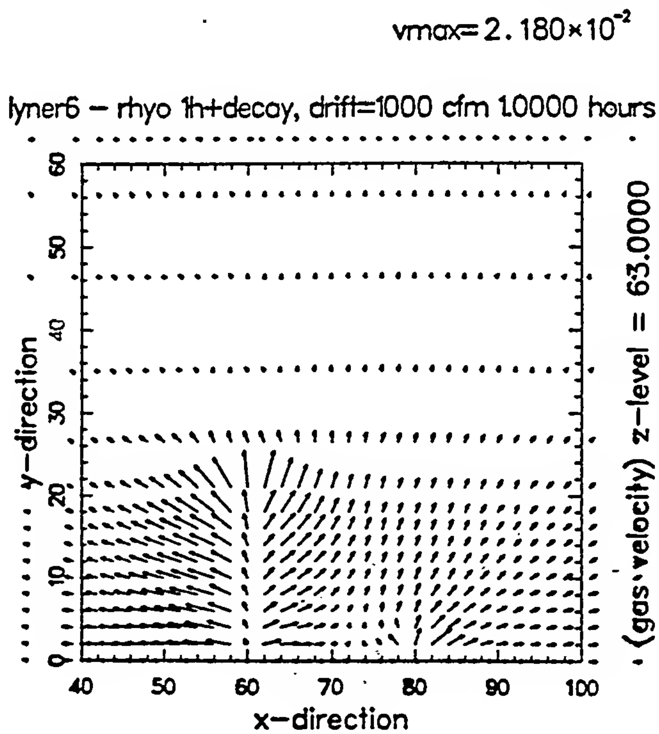


Figure 10c. Velocity vectors at 1 hour after detonation for the case of tunnel pressurization starting at shot time (Mod6).

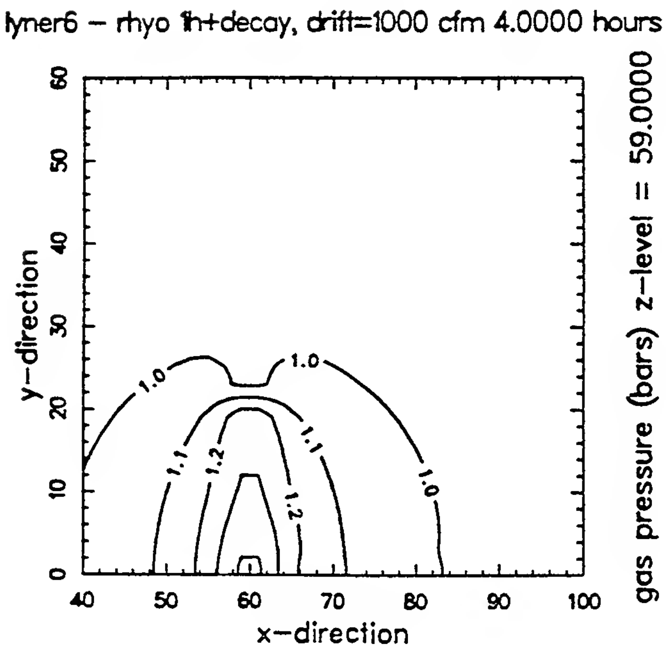


Figure 11a. Pressure contours (to 1.9 bars) at 4 hours after detonation for the case of tunnel pressurization starting at shot time (Mod6).

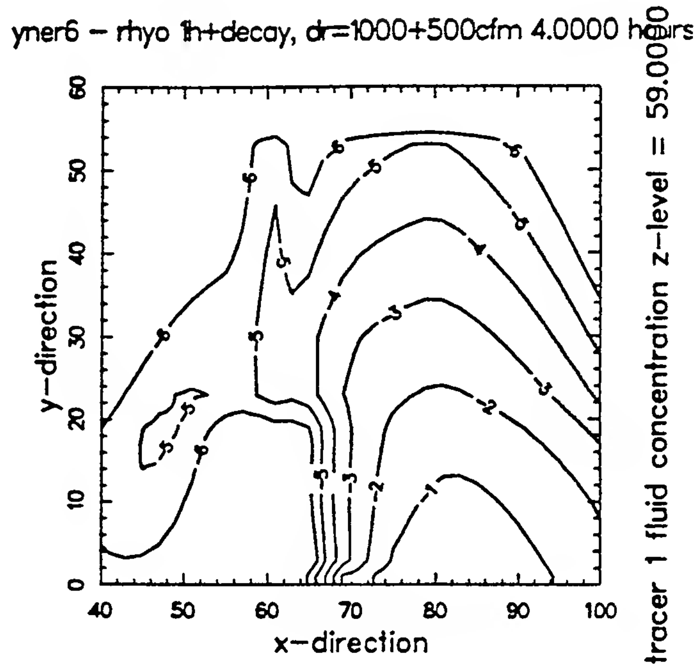


Figure 11b. Contamination contours (order-of-magnitude) at 4 hours after detonation for the case of tunnel pressurization starting at shot time (Mod6).

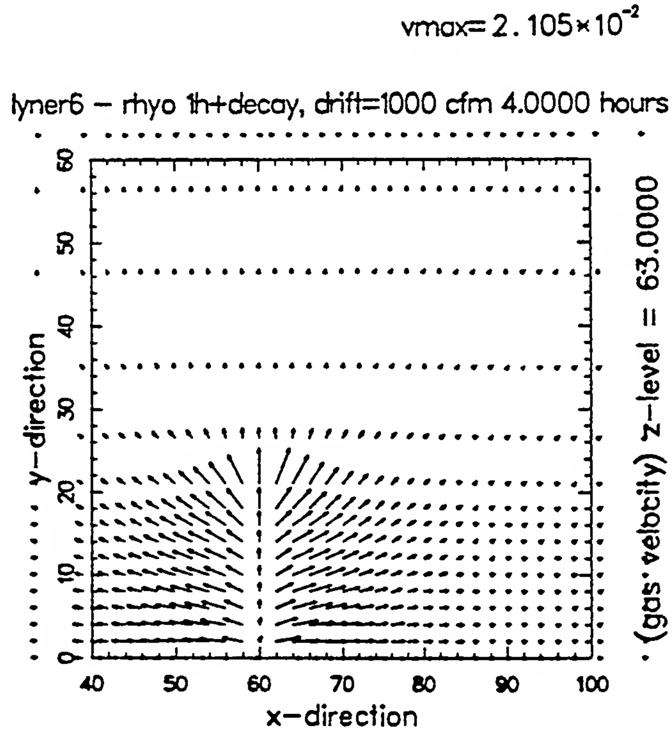


Figure 11c. Velocity vectors at 4 hours after detonation for the case of tunnel pressurization starting at shot time (Mod6).

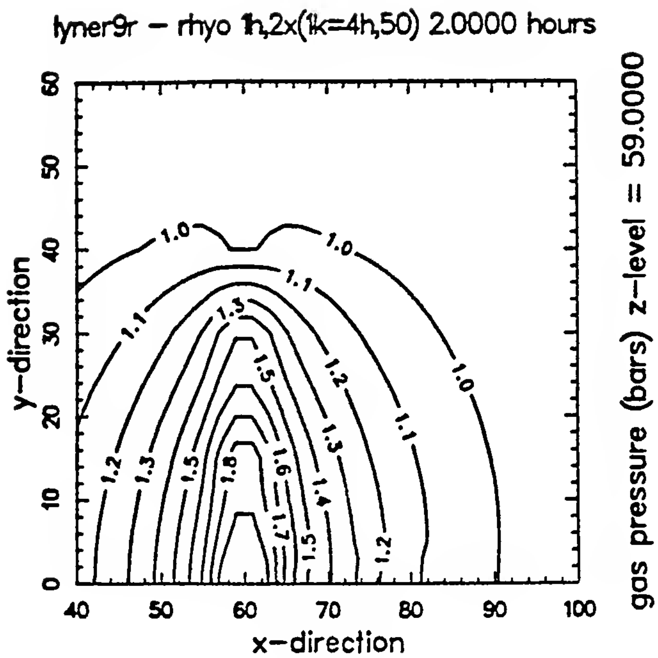


Figure 12a. Pressure contours (to 1.9 bars) after 2 hours of pre-pressurization but prior to detonation (Mod9 and Mod 10).

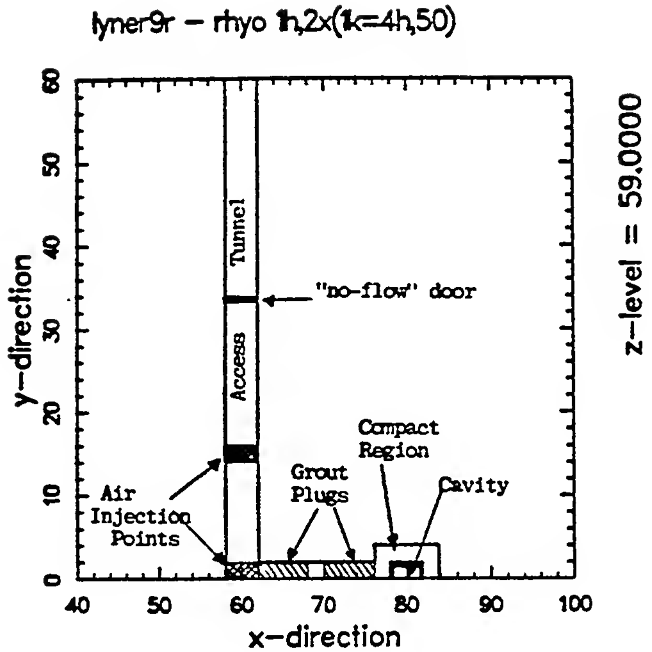


Figure 12b. Diagram of containment features and pressure sources including grout plugs and "no-flow" door (Mod 9 and Mod 10).

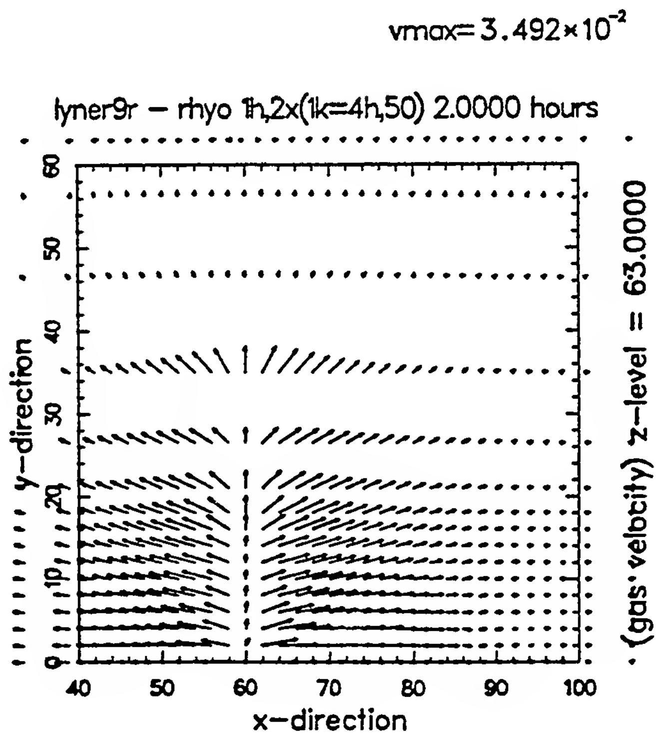


Figure 12c. Velocity vectors after 2 hours of pre-pressurization but prior to detonation (Mod9 and Mod 10).

the drift. The location of the important containment features are shown in Fig. 12b. The location of the "no-flow" door is 14m further back (from the plane of symmetry) than for the previous case (Mod 6). The velocity vectors after 2 hours are shown in Fig. 12c, illustrating the flow away from the drift in every direction.

The simulated detonation time is 2 hours; thus the times on all the figures will be 2 hours ahead of Figs. 8a to 8c, for example. Five minutes after detonation, the pressure field is shown in Fig. 13a. Notice that a large volume of alluvium is still under the influence of the pre-pressurization process, even though the highest pressures are clearly coming from the cavity. The contamination contours in Fig.13b are virtually identical to those in Figs. 8b and 5b, as expected. The velocity vectors in Fig. 13c are clearly governed by the cavity, however, the drift pressure has an obviously beneficial effect. After 15 minutes (2.25 hours of total time), the pressure in the cavity has dropped significantly, as in the previous calculations, as illustrated in Fig. 14a. The concentration contours in Fig. 14b are the first clear indication that the concept of pre-pressurization is useful. Notice that although the contamination is close to the drift, it has not passed through, unlike Figs. 9b and 6b. The fact that the pressure contours are equivalent in the drift and cavity is reinforced by the velocity vector plot in Fig. 14c. Here the air injection is pushing clean air toward the contaminated region, providing assurance that the drift will remain clean. The situation after one hour is shown in Figs. 15a to 15c. The pressures are now dominated by the drift effects and the radiation concentration contours are simply moving further away from the drift and the plane of symmetry. The reason for this is shown by the velocity vectors moving away from the drift.

The injection rate was cut to 800 cfm 2 hours after detonation (4 hours of total simulated time). The reasons for this are twofold. First, it is unnecessary and resource intensive to keep the blowers running at such a high rate after the first couple of hours. Secondly, as the injected air continues to push the contaminated gas away from the drift, previously uncontaminated regions of alluvium will be contaminated (at low levels) for no particularly good reason. Also, the air injection will tend to push contamination in every direction, including toward the surface. After four

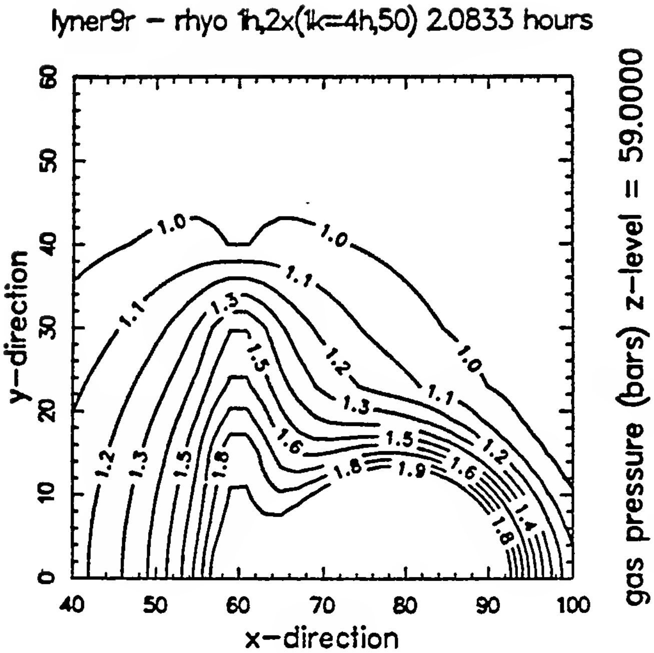


Figure 13a. Pressure contours (to 1.9 bars) at 5 minutes after detonation for the case of 2 hours of pressurization prior to shot time (Mod 9).

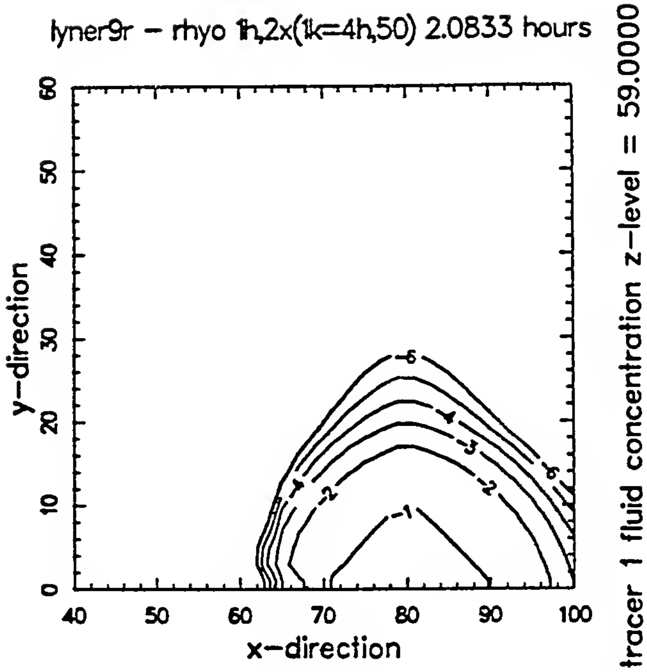


Figure 13b. Contamination contours (order-of-magnitude) at 5 minutes after detonation for the case of 2 hours of pressurization prior to shot time (Mod 9).

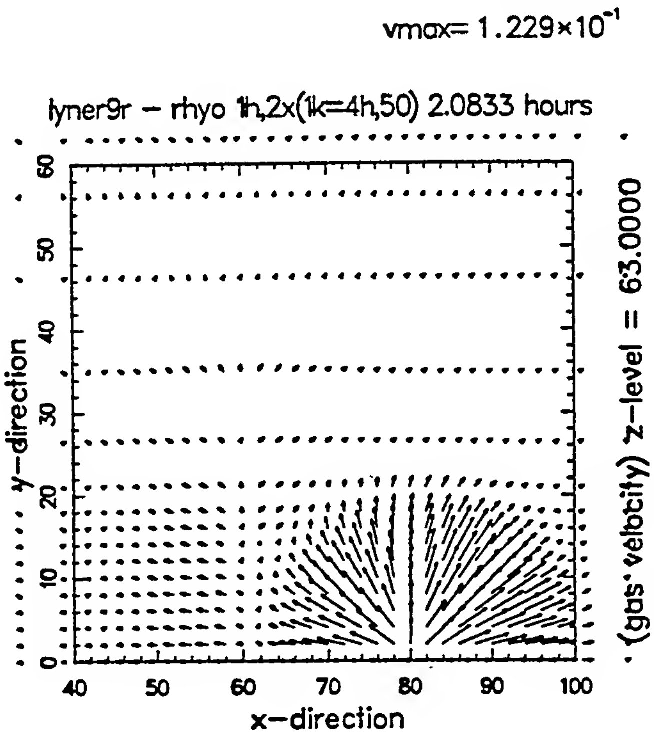


Figure 13c. Velocity vectors at 5 minutes after detonation for the case of 2 hours of pressurization prior to shot time (Mod 9).

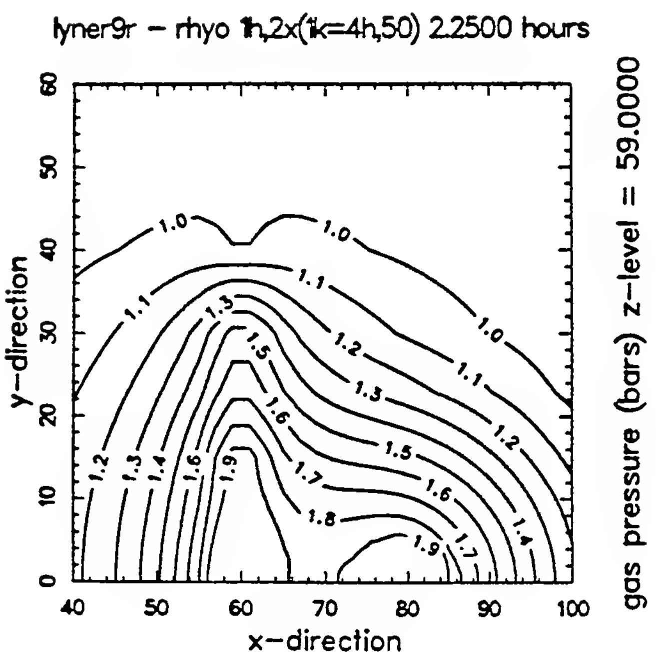


Figure 14a. Pressure contours (to 1.9 bars) at 15 minutes after detonation for the case of 2 hours of pressurization prior to shot time (Mod 9).

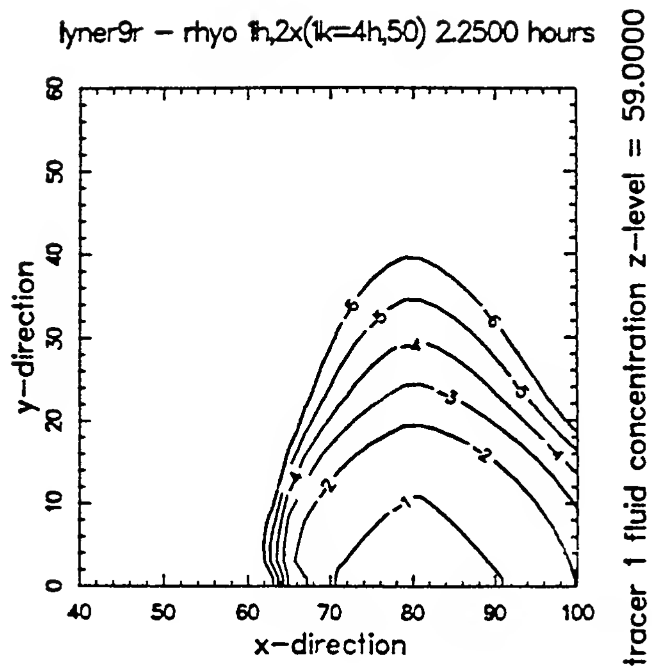


Figure 14b. Contamination contours (order-of-magnitude) at 15 minutes after detonation for the case of 2 hours of pressurization prior to shot time (Mod 9).

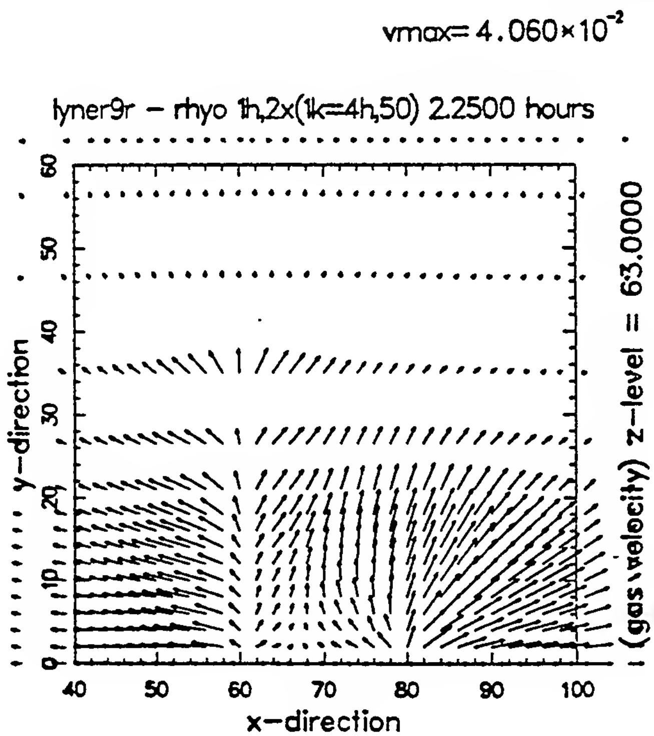


Figure 14c. Velocity vectors at 15 minutes after detonation for the case of 2 hours of pressurization prior to shot time (Mod 9).

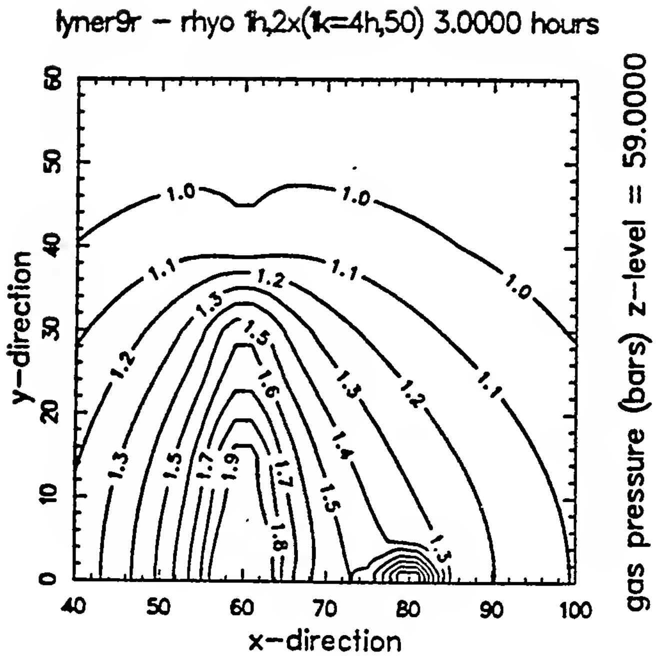


Figure 15a. Pressure contours (to 1.9 bars) at 1 hour after detonation for the case of 2 hours of pressurization prior to shot time (Mod 9).

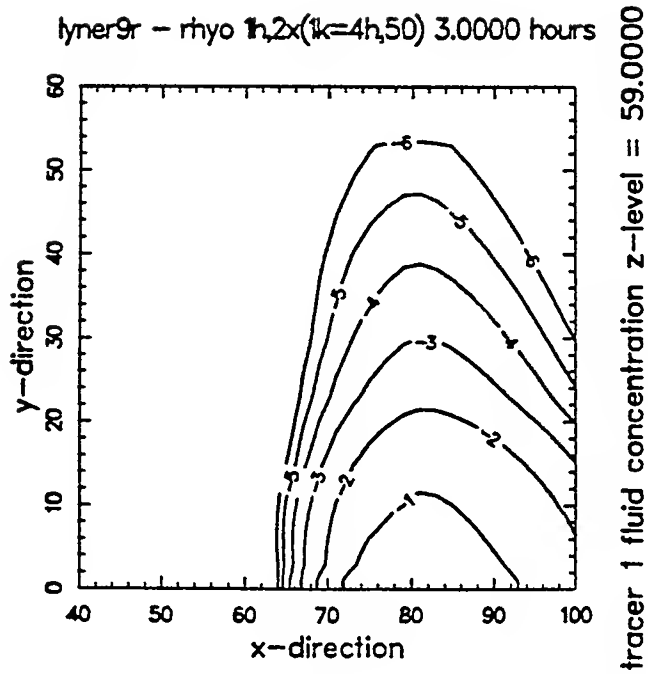


Figure 15b. Contamination contours (order-of-magnitude) at 1 hour after detonation for the case of 2 hours of pressurization prior to shot time (Mod 9).

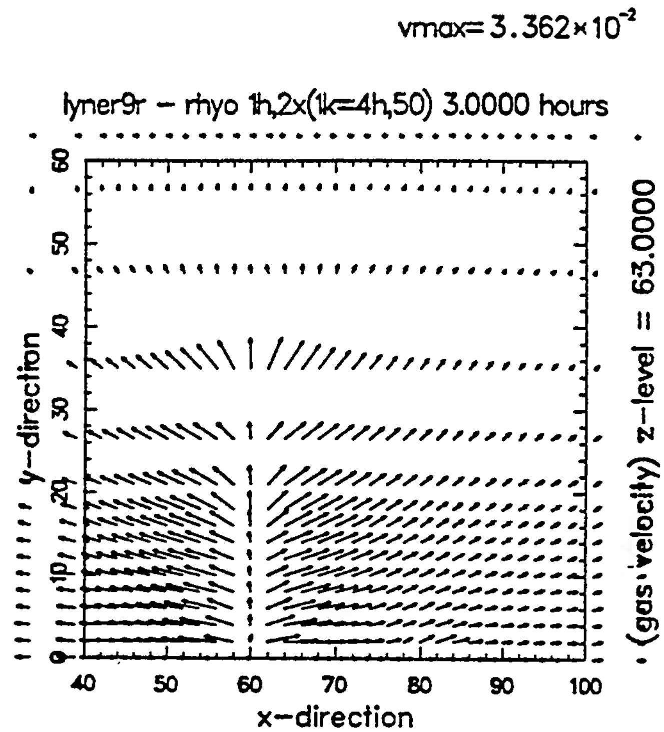


Figure 15c. Velocity vectors at 1 hour after detonation for the case of 2 hours of pressurization prior to shot time (Mod 9).

hours, the pressure has dropped in the formation as shown in Fig. 16a. The contamination has not changed much from Figs. 15b to 16b, but the fact that it is still moving may indicate that the 800 cfm injection is still too much, and perhaps a much smaller injection at these times is more appropriate. The velocities are clearly only reflecting the presence of the drift in Fig. 16c.

This calculation shows that the drift can be kept clean with the injection of moderate (by industrial standards) quantities of air. The contamination contours are very near the drift (and if one chooses to lower the threshold to more than the six orders-of-magnitude discussed earlier, some minor infiltration is observed). It is important to realize that the 20 m distance from the working point to the drift wall is probably much smaller than will be fielded, due to ground shock and hydrofracture constraints. This distance serves the porous flow calculations well, since it is a worst case, and the interaction of cavity pressures and drift pressures is more pronounced since the gas is forced to move in smaller regions. Also, calculational resources are tuned to modeling effects in high resolution, rather than using bigger zones (or many more zones) to get the exact dimensions right. Moreover, since the exact dimensions are far from being known at this time, this scale seems appropriate for the simulations presented in this appendix.

Drift Pre-Pressurization and the Effects of an Untamped Event

The pre-pressurization that was so effective in the tamped case discussed above was also used prior to detonation in a large room with untamped boundary conditions (Mod 10). A room size was selected which would be similar to the Ledoux experience (1100 cubic meters), but small enough that a new three-dimensional mesh would not have to be conceived, set up and debugged. Dimensions were selected to be 4 x 4 x 36 m long, for a total of 576 cubic meters. This zero room begins with the cavity used previously, i.e. 20 meters from the drift with a cross-section of 16 square meters, and extends 14 m into the coarsely zoned region (see Figs. 3a and 3b). Since we are interested in the effects near the drift, this seems reasonable. Again, it must be emphasized that we are looking only for qualitative effects.

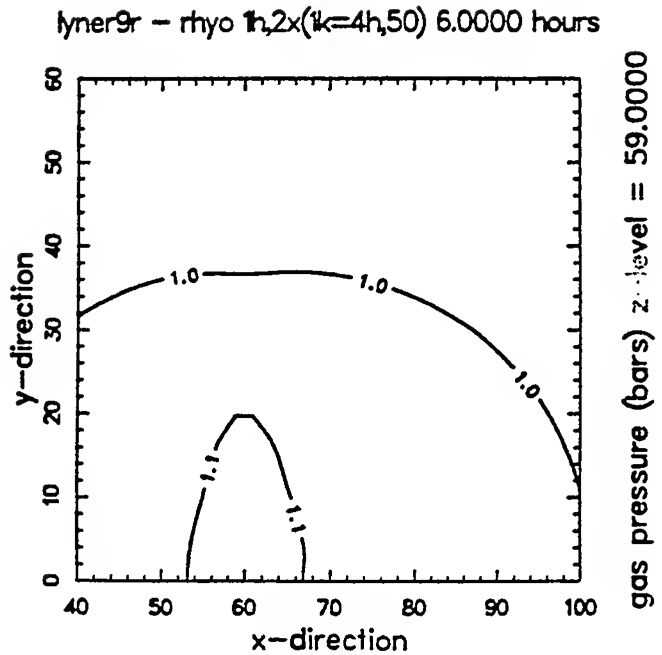


Figure 16a. Pressure contours (to 1.9 bars) at 4 hours after detonation for the case of 2 hours of pressurization prior to shot time (Mod 9).

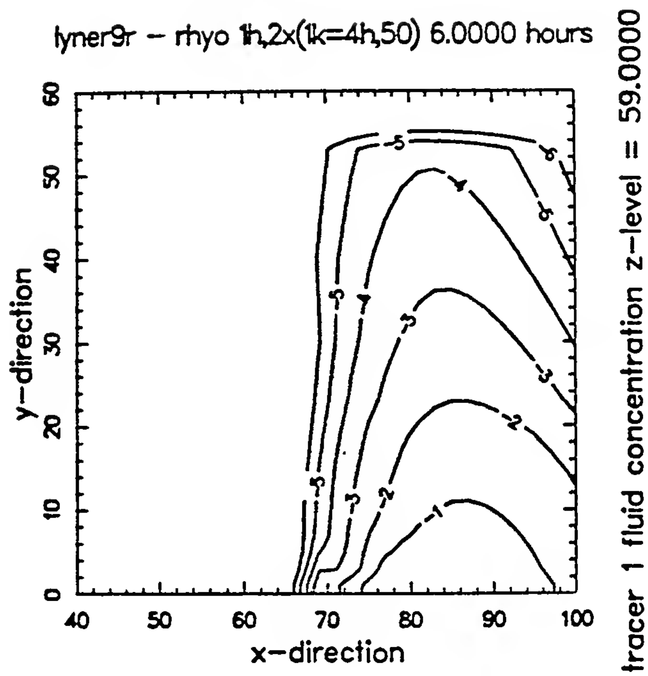


Figure 16b. Contamination contours (order-of-magnitude) at 4 hours after detonation for the case of 2 hours of pressurization prior to shot time (Mod 9).

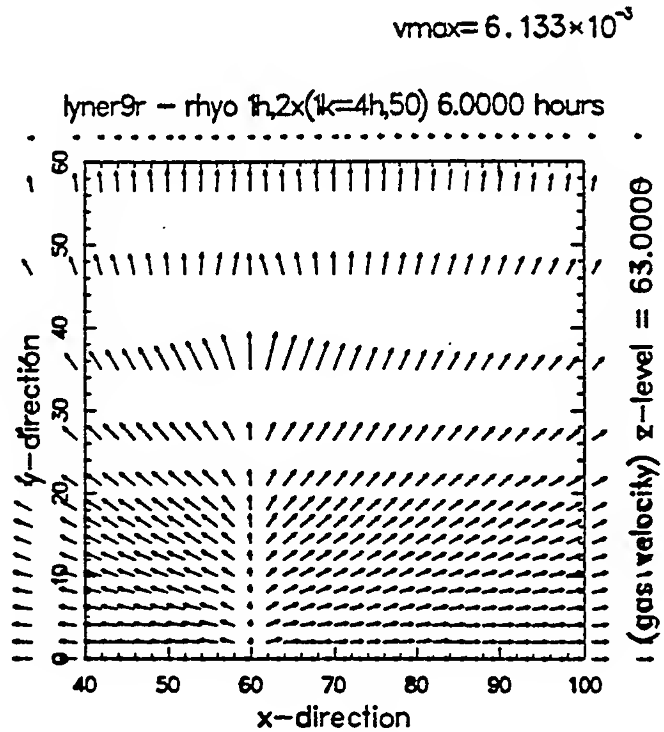


Figure 16c. Velocity vectors at 4 hours after detonation for the case of 2 hours of pressurization prior to shot time (Mod 9).

The pressure boundary condition selected was to assume that 20 roomfuls of noncondensable gas was generated and allowed to bleed off into the formation [3]. Everything else about this calculation was the same as Mod 9. The pressure and velocity fields just prior to detonation are therefore essentially as in Fig. 12a and 12c, respectively (although there was some minor effect of having a large, high-permeability room, along the axis of symmetry). The containment features are identical to those shown in Fig. 12b. The zero room extends from the cavity, 36 m off to the right. The pressure field five minutes after detonation is shown in Fig. 17a. Notice the similarity to Fig. 13a for the tamped event, except that the high pressure region extends off to the right of what has been plotted in Fig. 17a, rather than the spherical contours for the tamped case. The contamination contours in Fig. 17b are also comparable, but they have not traveled as far toward the drift as in Fig. 13b. This is probably due to the high pressures (67 bars) that drive the pressure at very early times in the tamped case. The velocity vectors in Fig. 17c show the dominating influence of the pressurized room. As before, at 15 minutes, the pressures have dropped considerably, and the drift and room pressures are roughly equivalent, as shown in Fig. 18a. The contamination continues to flow away from the plane of symmetry in Fig. 18b, but is effectively inhibited from crossing into the region of the drift. The velocity vectors in Fig. 18c show why this is happening: the flow from the drift is pushing gas away. A stagnation point can be observed approximately 7 m from the drift and 13 m from the room wall along the plane of symmetry.

One hour after shot time, the pressures in the room have almost completely dissipated, as illustrated in Fig. 19a. The contamination continues to move away from the room, but not toward the drift, as shown in Fig. 19b. The velocity vectors in Fig. 19c show a minor effect from the room. As before, the injection into the drift was cut from 4000 cfm to 800 cfm, 2 hours after detonation. The situation after four hours is illustrated in Figs. 20a to 20c. The pressures are lower, due to the decreased flow rate in the drift, and the contamination contours have continued to migrate away from the room (but not toward the drift). The velocity field is now completely governed by the injected air in the drift.

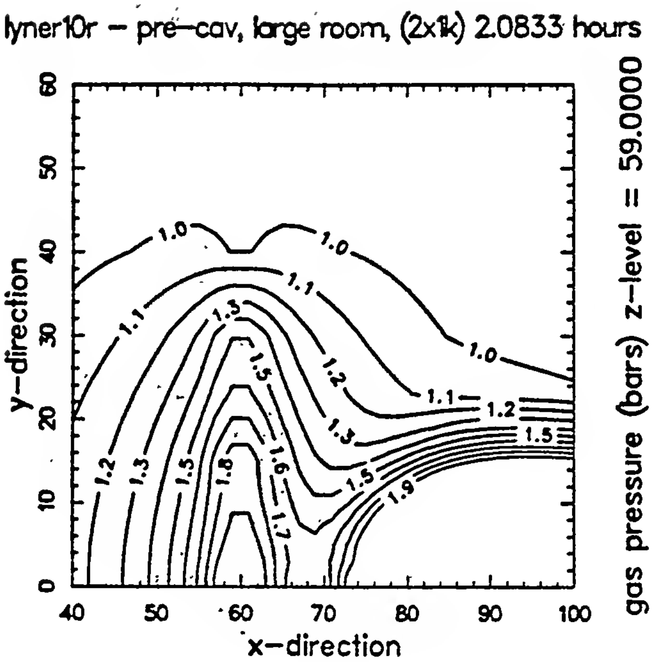


Figure 17a. Pressure contours (to 1.9 bars) at 5 minutes after detonation for the untamped case with 2 hours of pre-pressurization (Mod10).

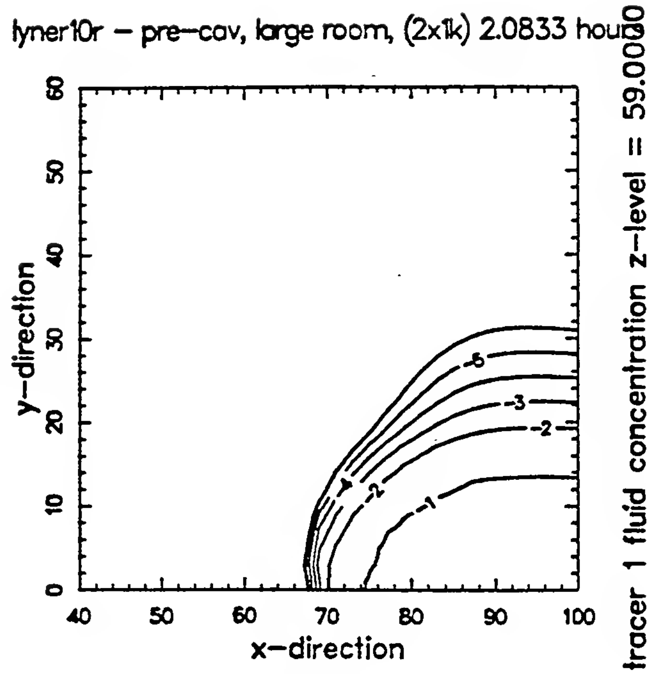


Figure 17b. Contamination contours (order-of-magnitude) at 5 minutes after detonation for the untamped case with 2 hours of pre-pressurization (Mod10).

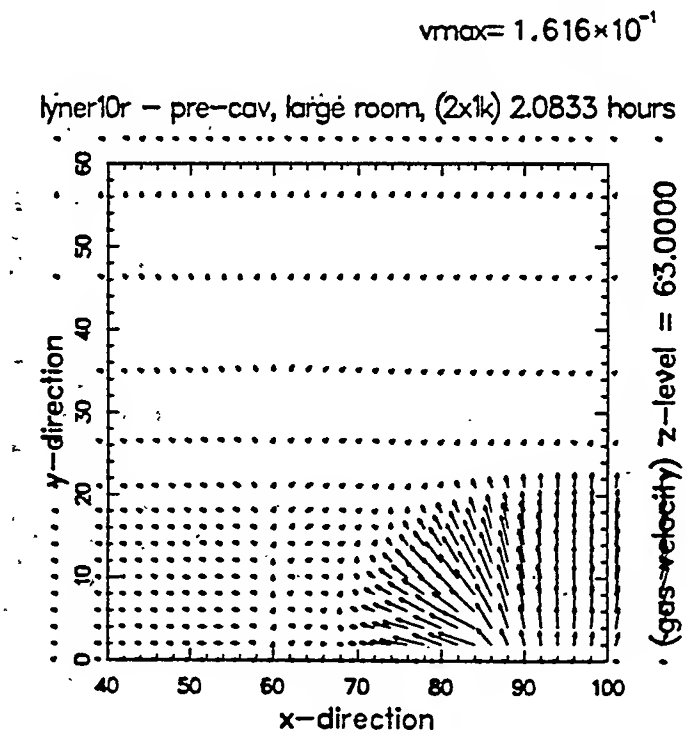


Figure 17c. Velocity vectors at 5 minutes after detonation for the untamped case with 2 hours of pre-pressurization (Mod10).

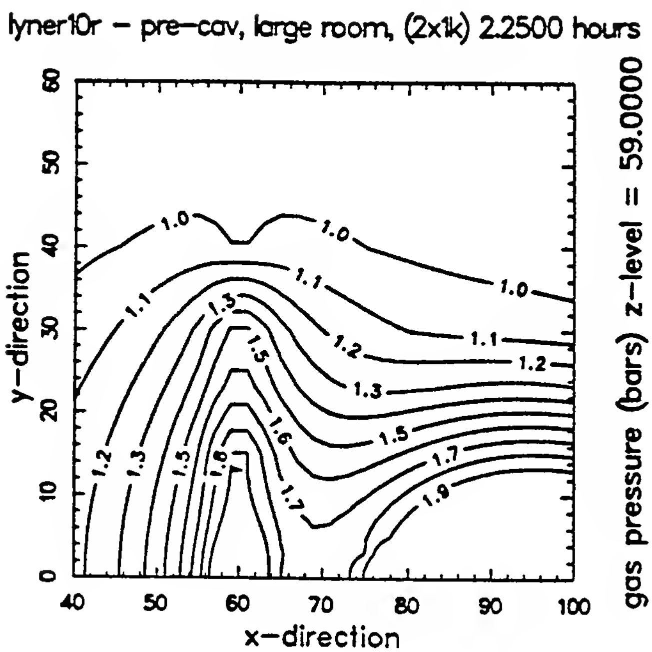


Figure 18a. Pressure contours (to 1.9 bars) at 15 minutes after detonation for the untamped case with 2 hours of pre-pressurization (Mod10).

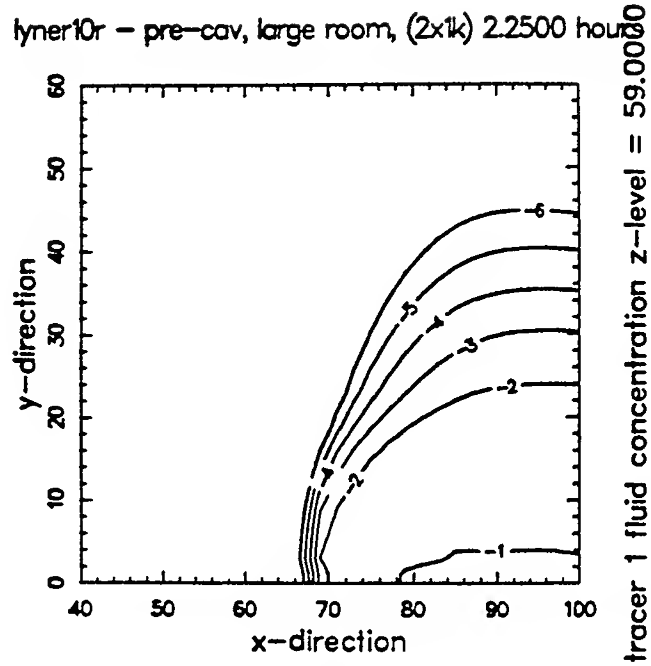


Figure 18b. Contamination contours (order-of-magnitude) at 15 minutes after detonation for the untamped case with 2 hours of pre-pressurization (Mod10).

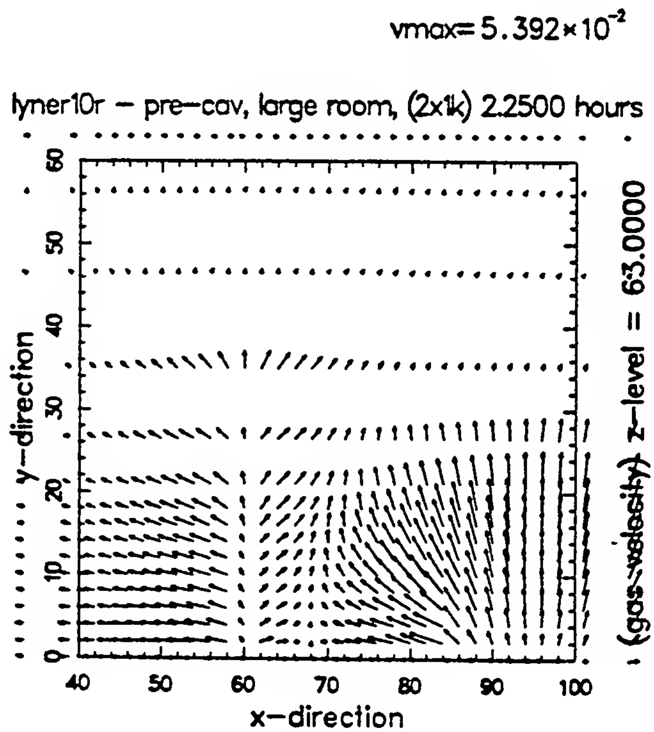


Figure 18c. Velocity vectors at 15 minutes after detonation for the untamped case with 2 hours of pre-pressurization (Mod10).

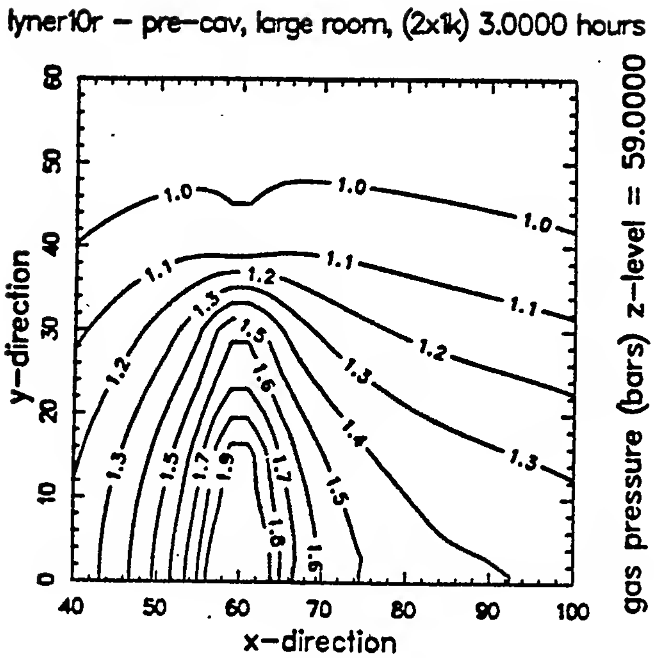


Figure 19a. Pressure contours (to 1.9 bars) at 1 hour after detonation for the untamped case with 2 hours of pre-pressurization (Mod10).

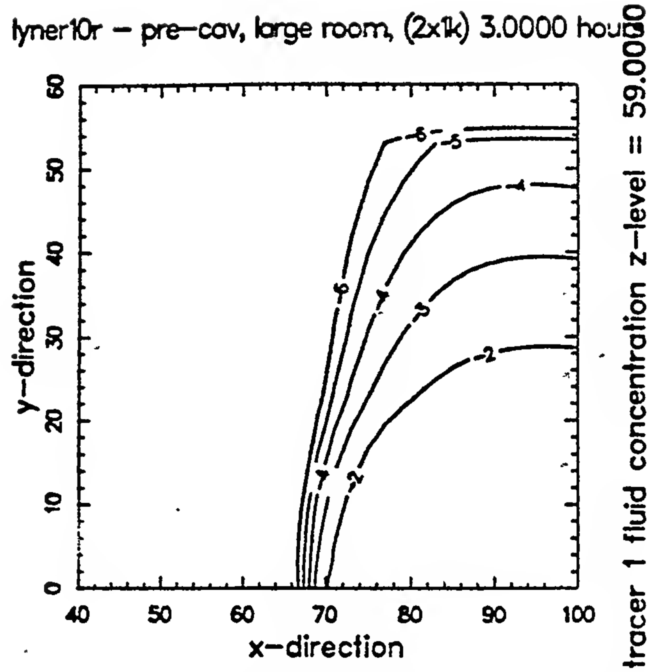


Figure 19b. Contamination contours (order-of-magnitude) at 1 hour after detonation for the untamped case with 2 hours of pre-pressurization (Mod10).

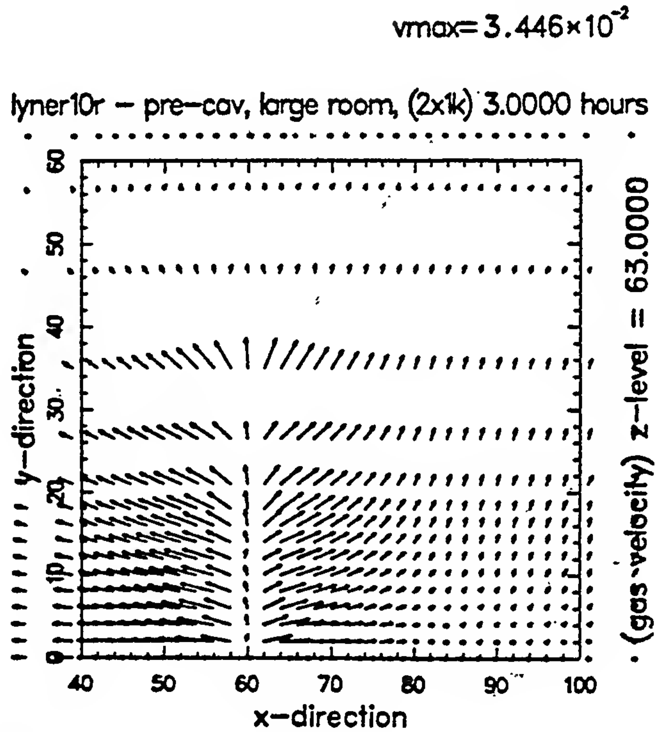


Figure 19c. Velocity vectors at 1 hour after detonation for the untamped case with 2 hours of pre-pressurization (Mod10).

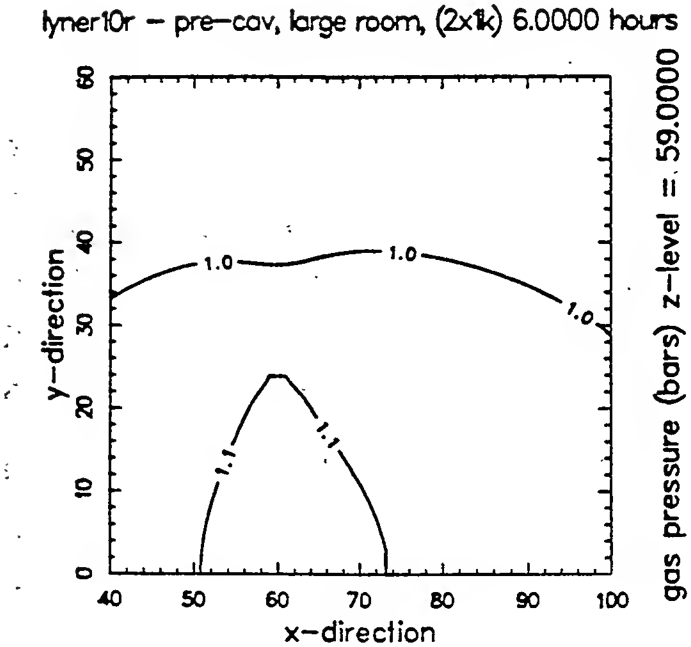


Figure 20a. Pressure contours (to 1.9 bars) at 4 hours after detonation for the untamped case with 2 hours of pre-pressurization (Mod10).

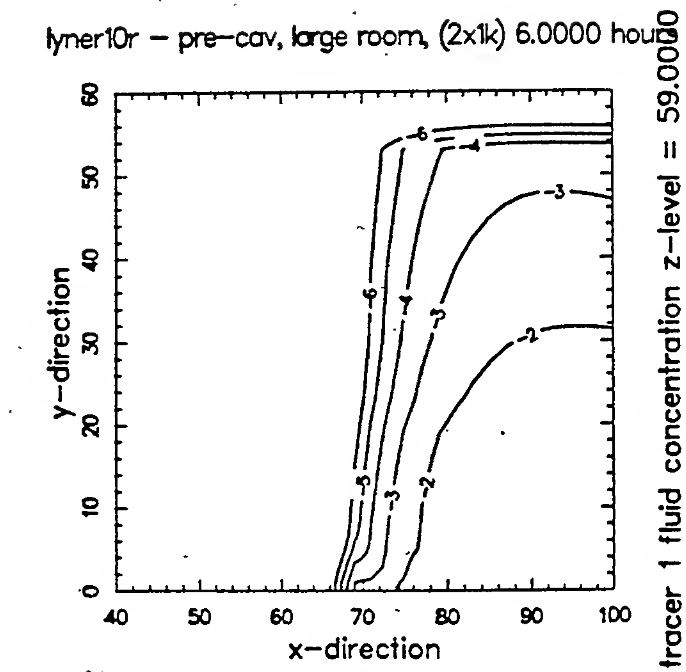


Figure 20b. Contamination contours (order-of-magnitude) at 4 hours after detonation for the untamped case with 2 hours of pre-pressurization (Mod10).

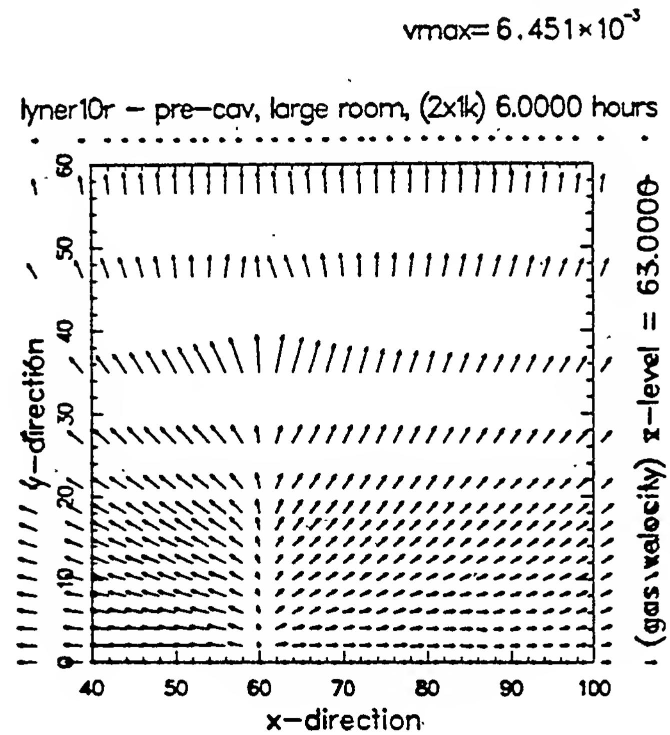


Figure 20c. Velocity vectors at 4 hours after detonation for the untamped case with 2 hours of pre-pressurization (Mod10).

This calculation has demonstrated qualitatively that the same general effects of pre-pressurization for tamped events will provide similar benefits for untamped events, as far as porous flow is concerned. This is, of course, strongly dependent on the assumptions we have made, in particular the pressure boundary condition of 20 bars, decaying into the formation. It is likely that the hot gases generated in the room will have an effect on the permeability of the walls, both because of possible glazing and water condensing in the pore spaces in the alluvium.

Time Histories of Pressure in the Cavity, Drift and the Alluvium

A time history of the pressure in the cavity is given in Fig. 21a for the case where pressurization began at shot time (Mod 6). The dashed line is actual data from a tamped event which was used as the driving pressure. The solid line is the pressure obtained from a post-processor, confirming that the pressure in the cavity was as prescribed. The cavity was allowed to bleed off into the formation after the data was no longer available, approximately 3800 seconds. Notice the tapering off of the pressure after this time. The reason the solid line begins at 300 seconds is that this was the first edit (variable dump) requested. Also note the pressure increasing in the drift as air is injected. Eventually, the pressure is greater than the cavity pressure, but not until the drift has been contaminated at early time.

Figure 21b shows the pressure history in the cavity for the baseline tamped case (Mod 9). The time scale is shifted by 7200 seconds, due to the 2 hour pre-pressurization sequence. The pressure from the drift has actually reached the cavity by shot time. The cavity pressure is prescribed by the measured data for the next hour or so, and then allowed to decay into the formation. A slight increase in pressure after zero time is observed in the drift, but since the contamination always lags the pressure front, no contamination passed into the drift. Also note the dramatic effect on the pressure 2 hours after shot time (time = 14400 seconds) reflecting the drop in injection rate from 4000 cfm to 800 cfm.

A time history of pressure in the zero room for the untamped case is given in Fig. 21c. Notice that the pressure does not build up quite as fast from the

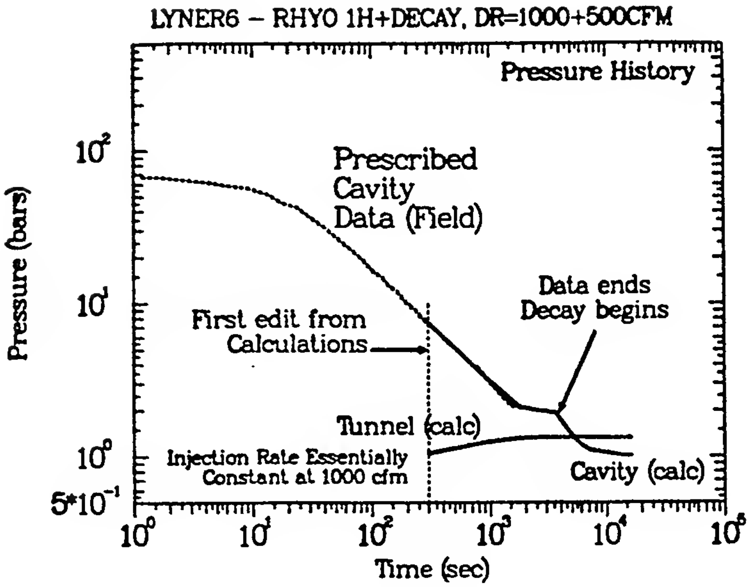


Figure 21a.

Pressure time histories for the case of tunnel pressurization starting at shot time (Mod 6). The dashed line is the prescribed (field data) history. The first edits from the calculation start at 300 seconds. Notice the point at which decay begins in the cavity and the pressure response in the tunnel.

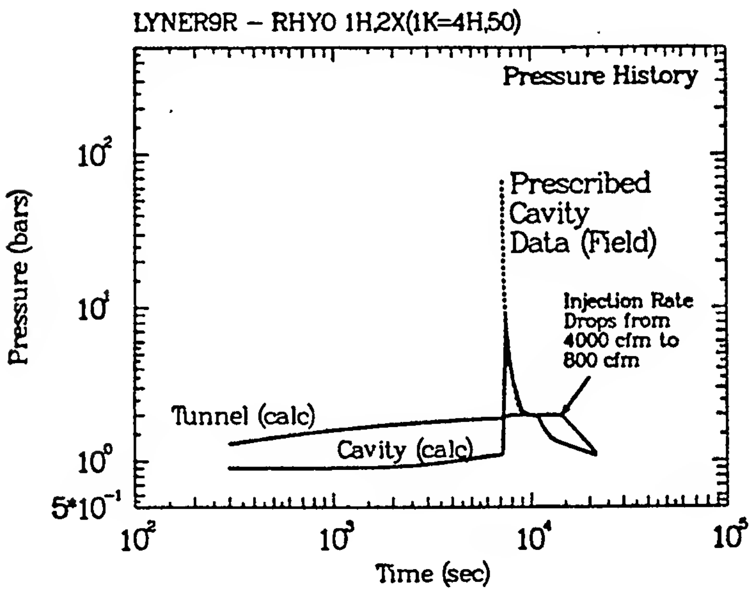


Figure 21b.

Pressure time histories for the case of 2 hours of pre-pressurization prior to shot time (Mod9). Notice that all the curves are shifted by 7200 seconds, compared to Fig. 21. Also notice the drop in pressure in the tunnel when the injection rate drops from 4000 to 800 cfm.

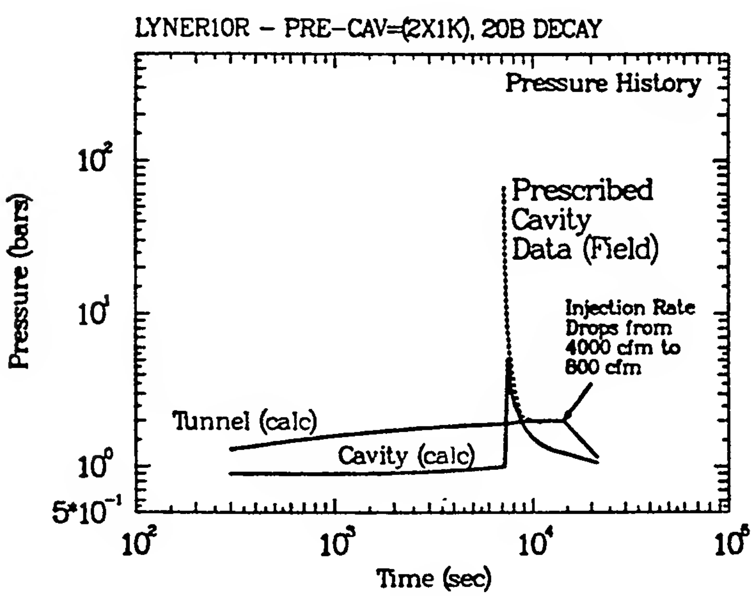


Figure 21c.

Pressure time histories for the untamped case with 2 hours of pre-pressurization (Mod10). The field data is shown for reference, but was not used in this calculation. The pressure in the zero room was set to 20 bars and allowed to decay in to the formation. Notice the slightly slower rate of pressurization in the zero room prior to detonation, compared to Fig. 22, reflecting the larger volume of the zero room compared to the cavity in Mod 9.

pre-pressurization, due to the much large volume of the room compared to the cavity. Also note that the pressure drops a bit more rapidly from the initial 20 bars in this case than when the cavity pressure was prescribed from field data in Fig. 21b.

Pressure histories at three field points in the alluvium for these three cases were also taken. The three points are y=10 m, and x=65, 75 and 85 m. All three points are 10 m from the plane of symmetry, and the x-values correspond to near the drift, near the cavity and directly over the cavity, respectively. Figure 12b shows the dimensions of the region. The pressure histories for the case where drift pressurization began at shot time are shown in Fig. 22a. The times have been shifted 2 hours, so as to have comparable scales as Figs. 22b and 22c. The axes have been scaled linearly, to show detail, but the times are the same as in Figs. 21a to 21c. Notice the pressure in the alluvium nearest the drift (x=65 m) is primarily affected by the environment in the drift and does not really see the cavity. The other two locations reflect the cavity pressurization, with the point directly above the cavity (x=85 m) showing a slightly faster drop in pressure at late times, due to its distance from the pressurized drift.

Figure 22b shows time histories at these same points, for the case where the drift was pressurized for 2 hours prior to shot time. All the points show the influence of this pressure. The thicker line is nearest to the drift, and shows a significant increase in pressure. The pressure wave from the cavity rises fairly slowly, and then tapers off. The injection rate change in the drift is clearly reflected in this curve. The thin line is near the drift while the dashed line is directly over the cavity. As before, the pressure drops more rapidly over the cavity.

The pressures in the alluvium for the untamped case are shown in Fig. 22c. Here, the response is similar to the previous case, however the pressure directly over the cavity rises to a higher value, and the intermediate point shows a more moderate increase at a slower rate. It is interesting to note that the greatest pressure increase in the alluvium is less than 2 bars over ambient conditions for all the cases, even when the pressure in the cavity started at 67 bars and the zero room was initially at 20 bars. This reflects the

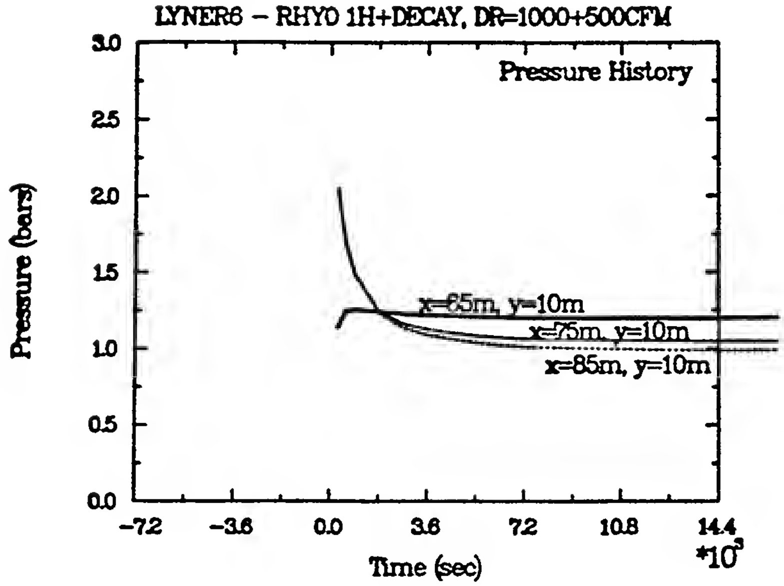


Figure 22a.

Pressure time histories for three points in the alluvium for the case of tunnel pressurization starting at shot time (Mod 6). The x-axis has been shifted by 2 hours to compare to similar plots for Mod9 and Mod10. Notice that the effect of tunnel pressurization has very little effect on points away from the tunnel until very late time.

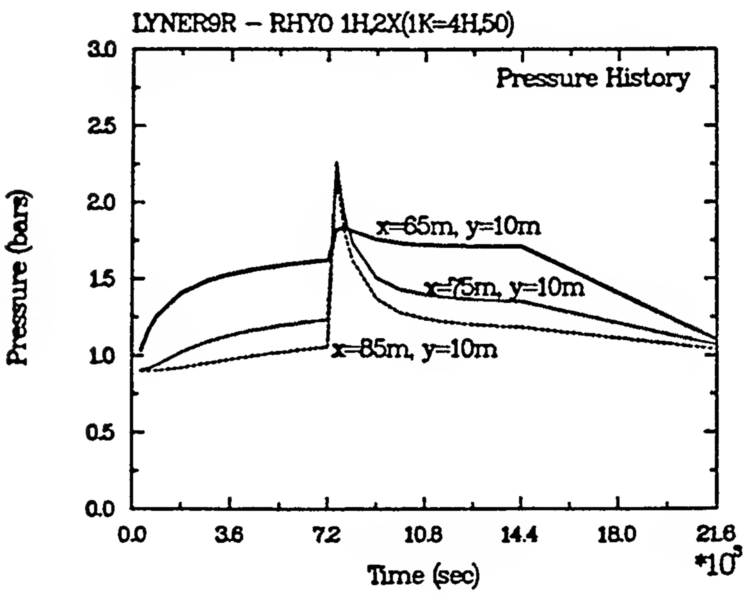


Figure 22b.

Pressure time histories for three points in the alluvium for the case of 2 hours of pre-pressurization prior to shot time (Mod9). The thicker line is closest to the tunnel and the dashed line is directly over the cavity. Notice the gradual rise in pressure at all points and the sharp rise in pressure for the two points near the cavity.

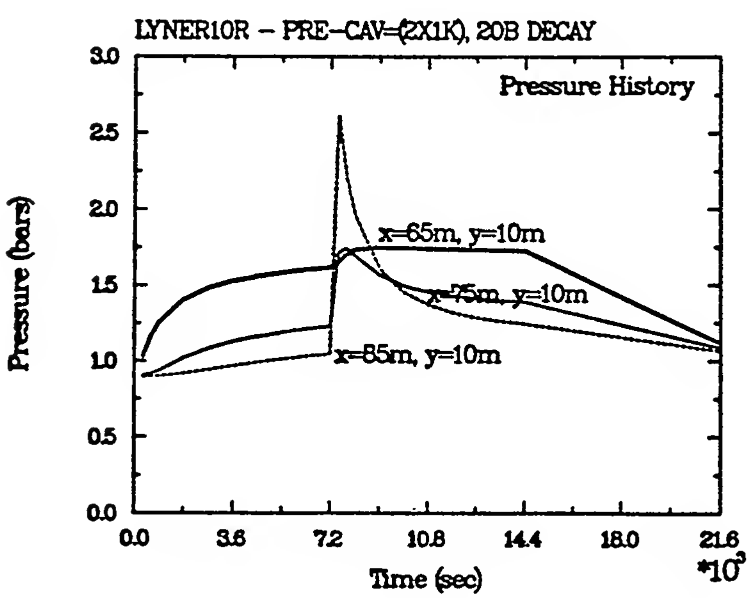


Figure 22c.

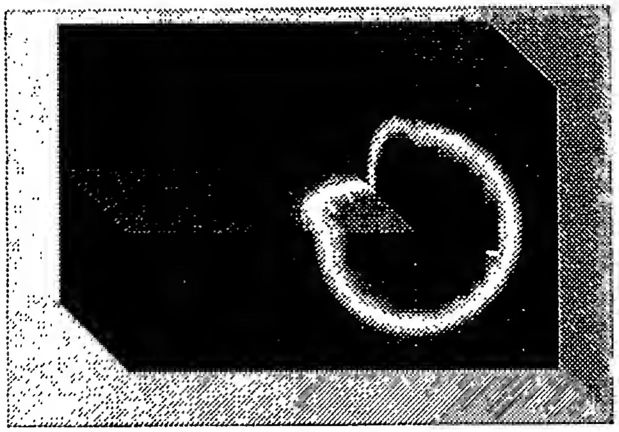
Pressure time histories for three points in the alluvium for the untamped case with 2 hours of pre-pressurization (Mod10). Here the pressure rise above the zero room (dashed line) is somewhat higher than in the previous case, but the intermediate point shows a more moderate peak and a slower rise time. The response at the point nearest the tunnel (thick line) is quite similar to the same curve in Fig. 22b.

relatively slow response of the porous flow process, compared to hydrofracture, for example. Recall that the first edit from the calculation was 300 seconds after detonation, so somewhat higher pressures were probably present during the first five minutes.

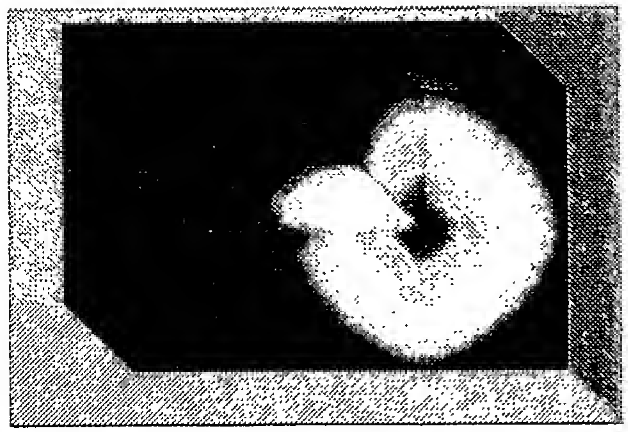
THREE-DIMENSIONAL VISUALIZATIONS

The discussion above has provided some insight into the expected phenomenology of underground experiments in a LYNER-like environment. The two-dimensional contour plots shown provide only a small fraction of the available information generated by the TRACR3D code. To provide a better sense of the tradeoffs between the four simulations discussed above, three-dimensional color renditions of pressure and contamination are provided in Figs. 23 to 27.

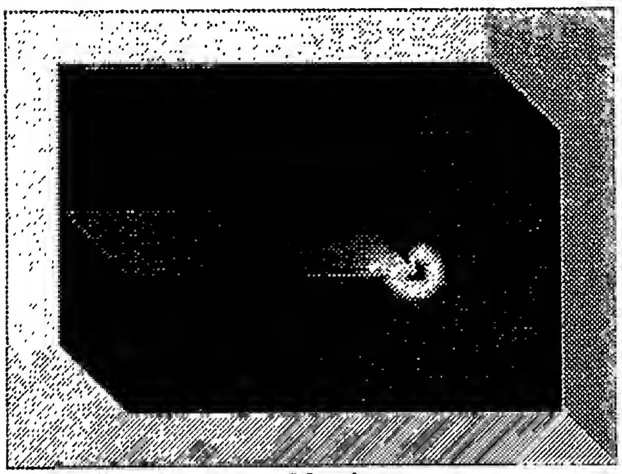
The dimensions of the mesh and the dominant containment features are given in Figs. 2 and 12b. Results from the case of no drift pressurization are shown in Fig. 23. Notice the pressure and contamination contours are essentially one-dimensional, with some minor effect of the low permeability grout plugs seen. The case of drift pressurization starting at shot time are shown in Fig. 24. Here, the contamination reaches the drift complex at early time, but is flushed out with clean air after 4 hours. Figure 25 shows the evolution of the pressure during the 2 hours of drift pre-pressurization. Notice the two injection points at early time. The results of the cavity effects are shown in Fig. 26. At early times the pressure field is dominated by the cavity pressures, but at later times, the drift pressures dominate. Finally, the same pressurization (Fig. 25) was applied prior to detonation of the untamped event. Although the early time pressures clearly show the long room, the eventual contamination pattern is very similar to the previous case. Note that contamination decay began almost immediately for this case, since the 20 bar pressure was allowed to bleed off into the formation right away. Decay did not begin until one hour for the tamped case, since the pressures and concentrations were specified for one hour. The important result for both these calculations is that no contamination reached the drift complex and was effectively kept at bay using moderate drift pressures. These results are clearly seen with the use of the three-dimensional graphics.



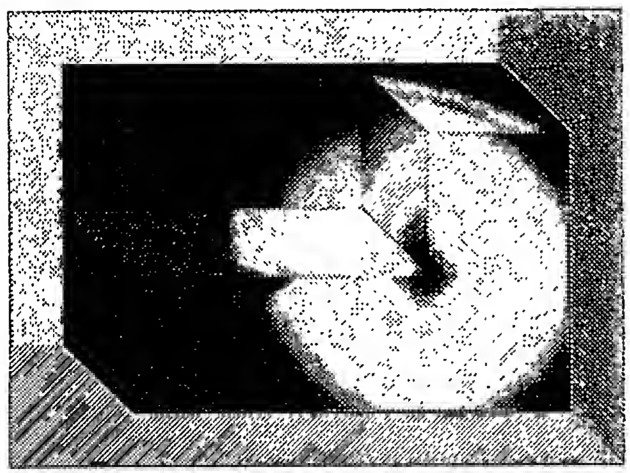
Pressure at t = 5 minutes
Max = 7.537b, Min = 0.900 b



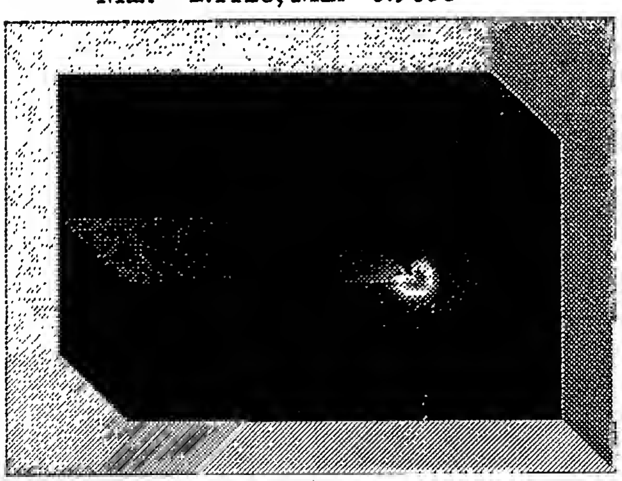
Log of tracer concentration at t = 5 minues
Max = 0, Min = -6



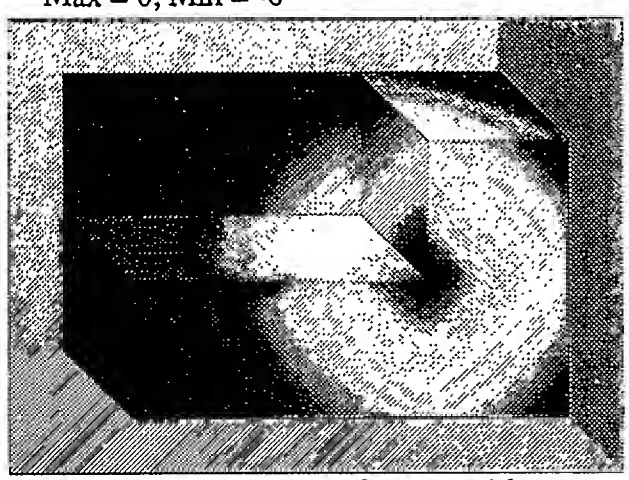
Pressure at t = 30 minutes
Max = 2.112b, Min =0.903b



Log of tracer concentration at t = 30 minutes
Max = 0, Min = -6

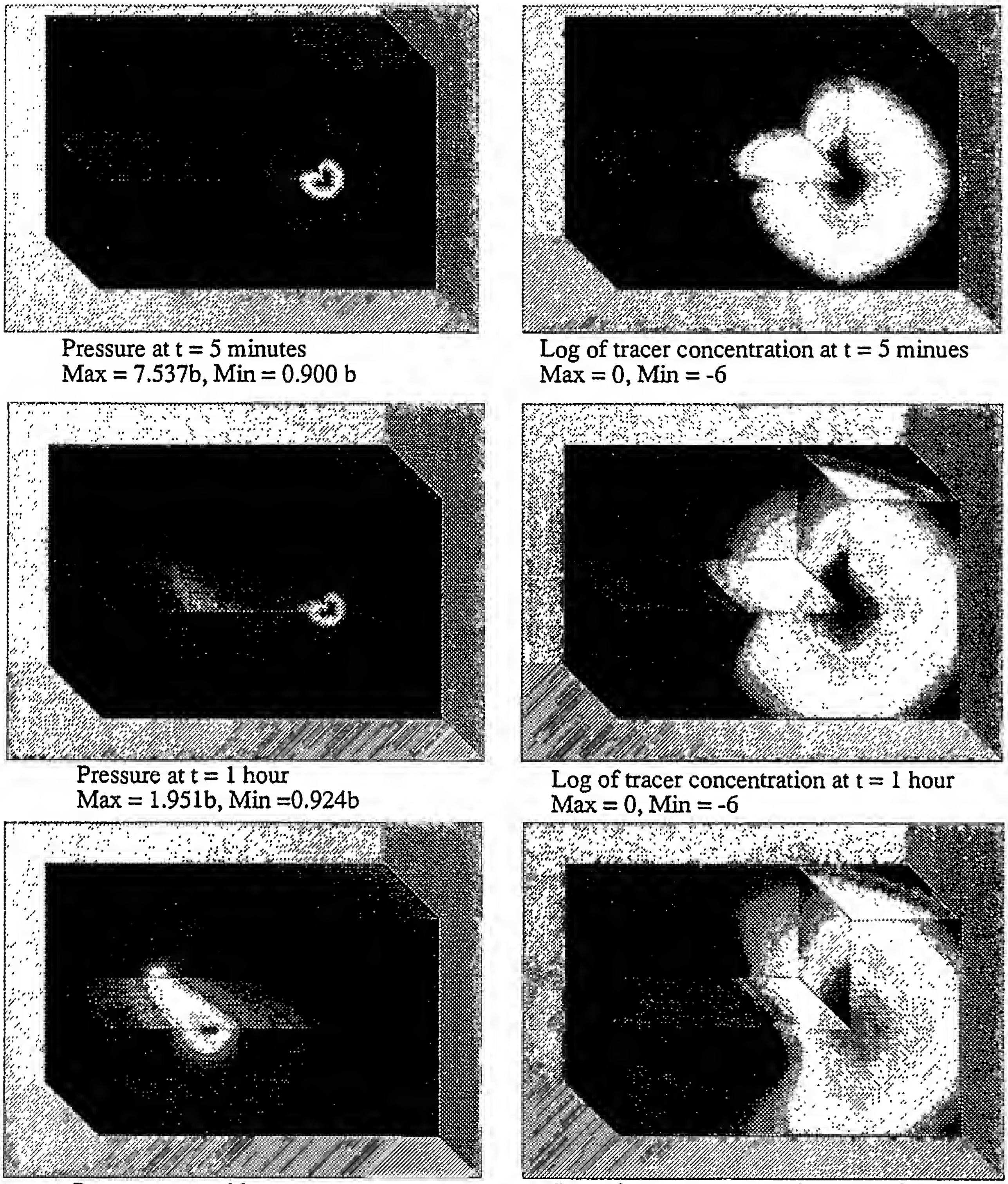


Pressure at t = 1 hour
Max = 1.951b, Min =0.918b



Log of tracer concentration at t = 1 hour
Max = 0, Min = -6

Figure 23. Three-dimensional color contours of pressure and tracer concentration for the case of no tunnel pressurization. (Mod. 5. Note all pressure plots are scaled to a Max = 2b and Min = 0.9b). See Fig. 2 for dimensions and location of containment features.



Pressure at t = 5 minutes
Max = 7.537b, Min = 0.900 b

Log of tracer concentration at t = 5 minues
Max = 0, Min = -6

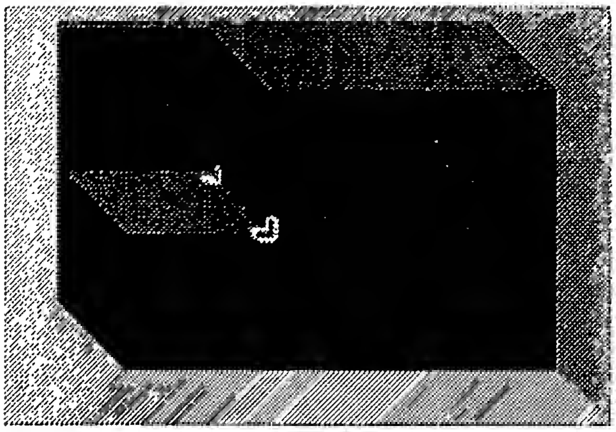
Pressure at t = 1 hour
Max = 1.951b, Min =0.924b

Log of tracer concentration at t = 1 hour
Max = 0, Min = -6

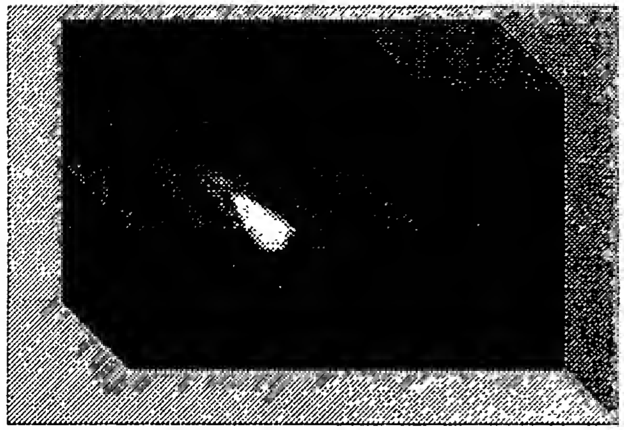
Pressure at t = 4 hours
Max = 1.417b, Min =0.933bb

Log of tracer concentration at t = 4 hours
Max = 0, Min = -6

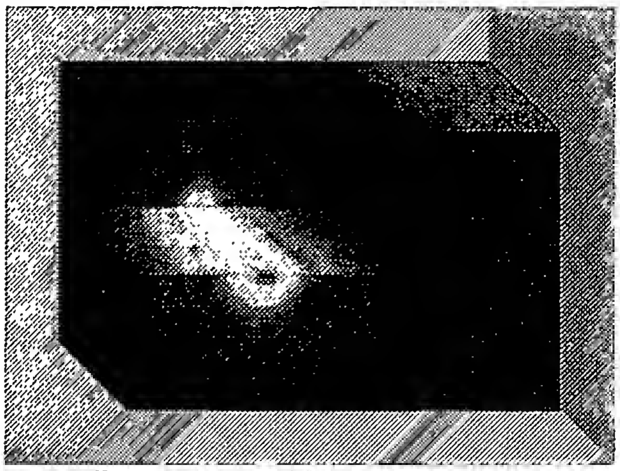
Figure 24. Three-dimensional color contours of pressure and tracer concentration for the case of tunnel pressurization starting at shot time (Mod 6). See Fig. 2 for dimensions and location of containment features.



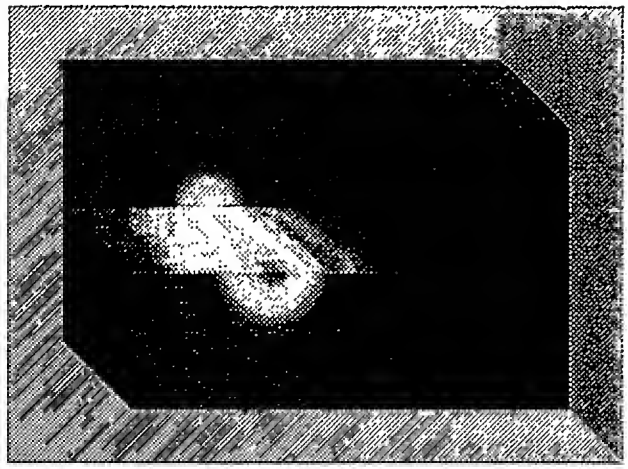
Pressure at t = 5.7 minutes
Max = 0.9008b, Min = 0.9000b



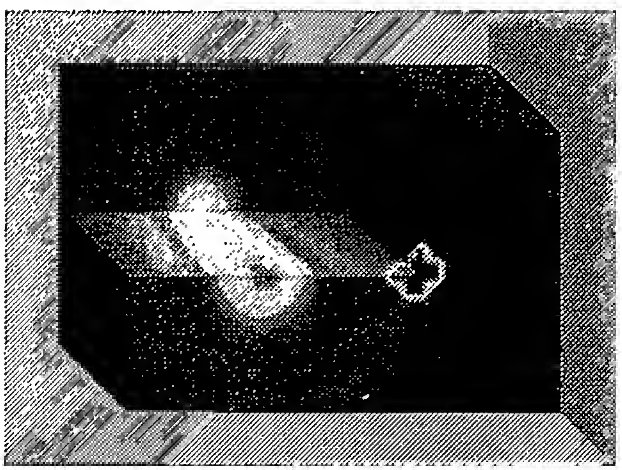
Pressure at t = 15 minues
Max =1.790b, Min =0.900b



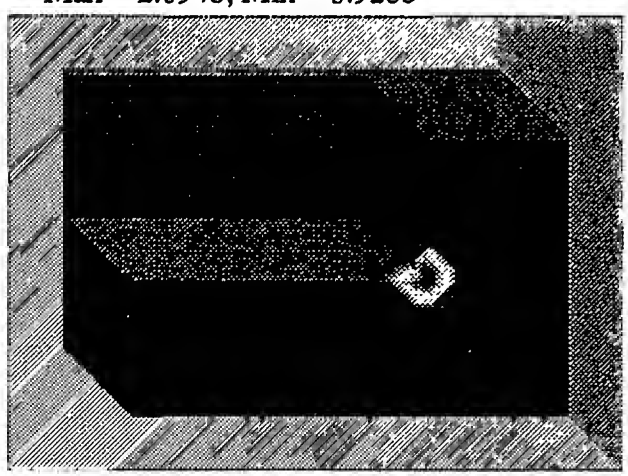
Pressure at t = 1 hour
Max = 2.021b, Min =0.9054b



Pressure at t = 2 hours
Max = 2.094b, Min = 0.928b

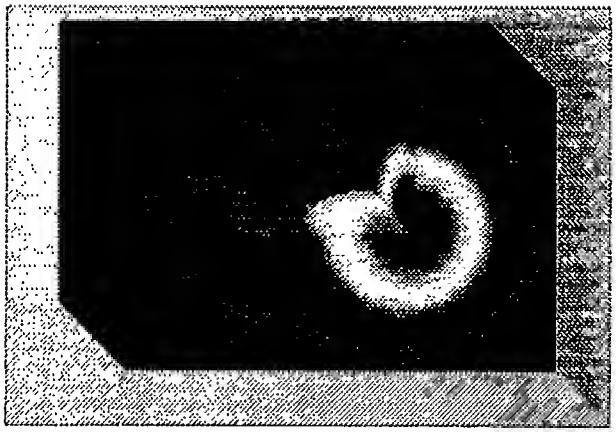


Pressure at t = 2 hours + .4 sec
Max = 67.16b, Min =0.928b

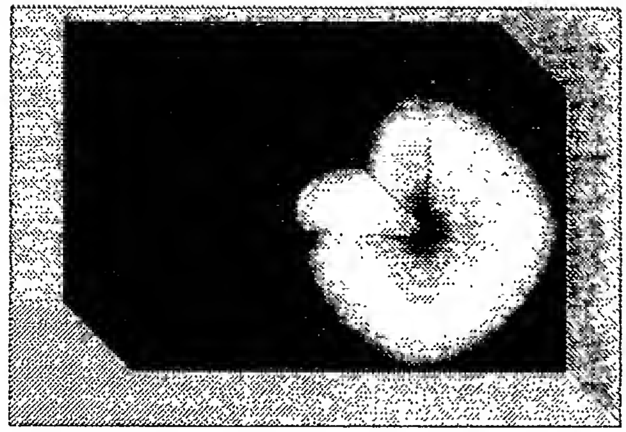


Log of tracer concentration at t = 2 hours + .4 sec
Max = 0, Min = -6

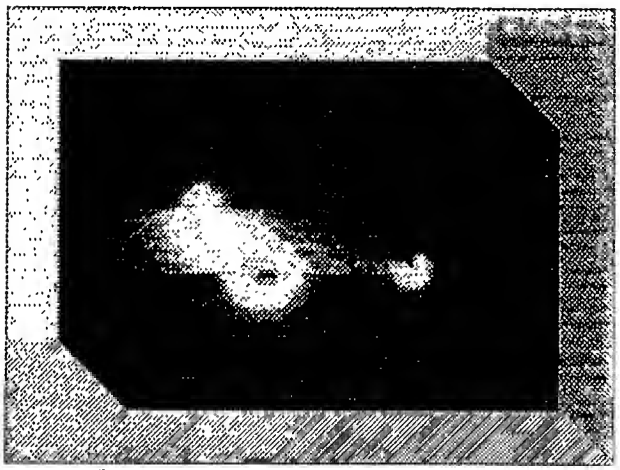
Figure 25. Three-dimensional color contours of pressure during the pre-pressurization phase (2 hours) for Mod. 9 and Mod. 10. See Fig. 2 for dimensions and location of containment features. (Note all pressure contours, except t = 5.7 sec, are scaled to max = 2 b and min = 0.9b.)



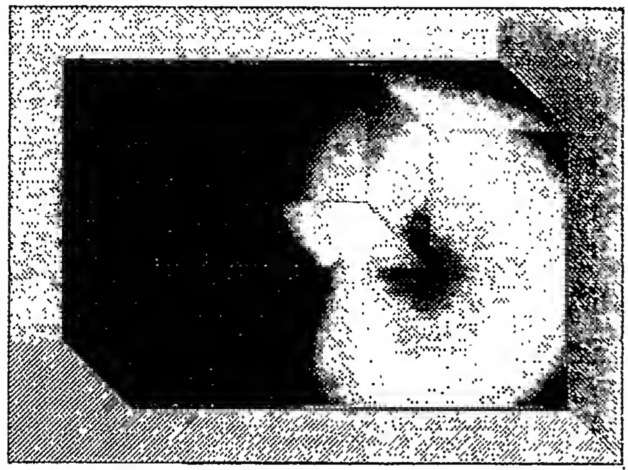
Pressure at t = 5 minutes
Max = 7.5376, Min = 0.930 b



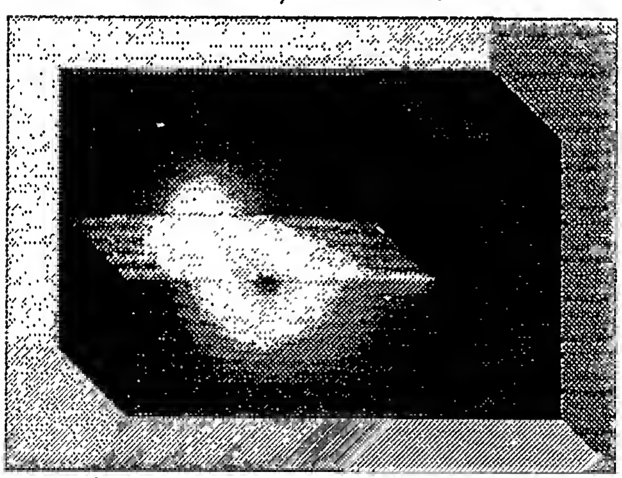
Log of tracer concentration at t = 5 minues
Max = 0, Min = -6



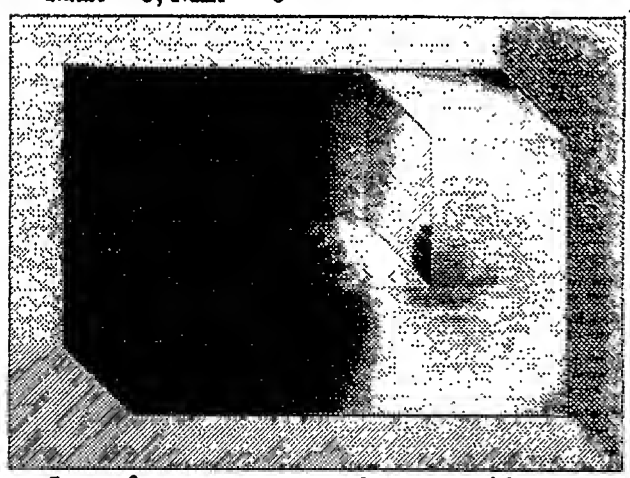
Pressure at t = 1 hour
Max = 2.168b, Min = 1.009b



Log of tracer concentration at t = 1 hour
Max = 0, Min = -6

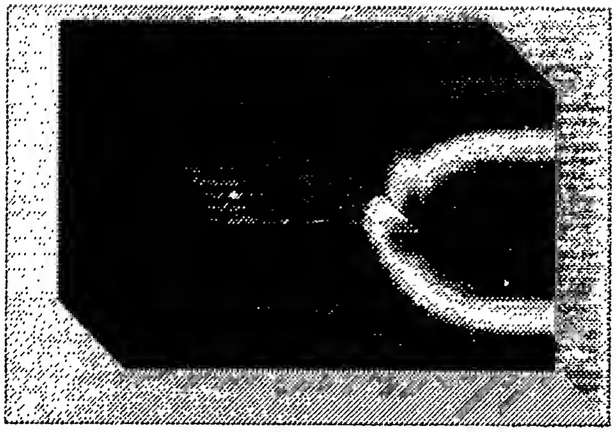


Pressure at t = 4 hours
Max = 1.144b, Min = 0.981b

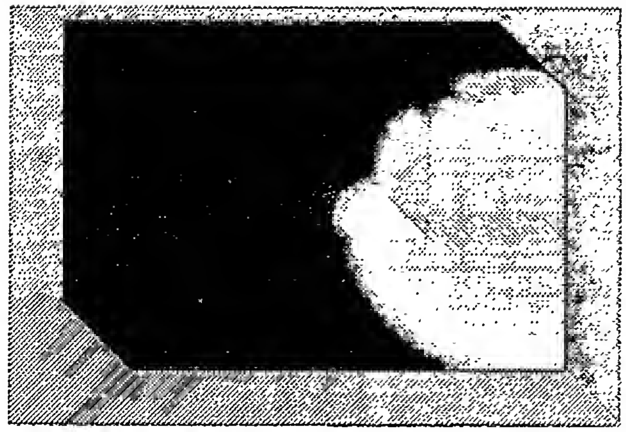


Log of tracer concentration at t = 4 hours
Max = 0, Min = -6

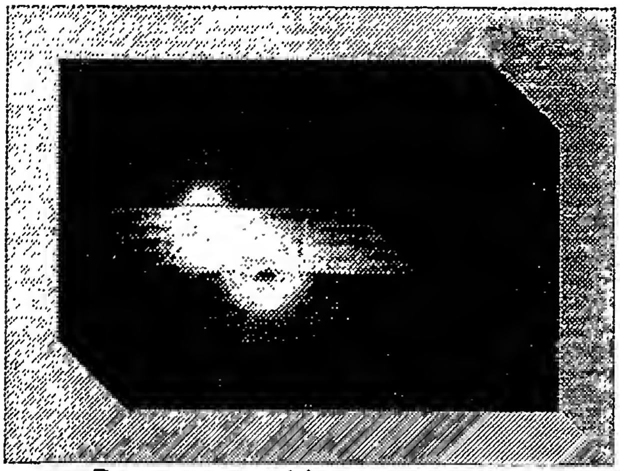
Figure 26. Three-dimensional color contours of pressure and tracer concentration for the case of 2 hours of pressurization prior to shot time (Mod. 9). See Figure 2 for dimensions and locations of containment features.



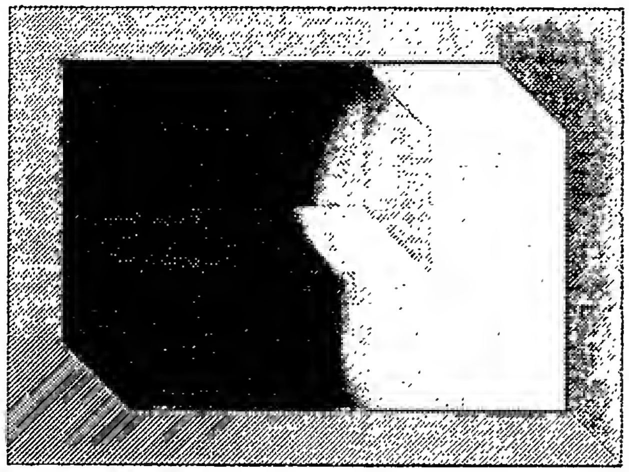
Pressure at t = 5 minutes
Max = 4.522b, Min = 0.943b



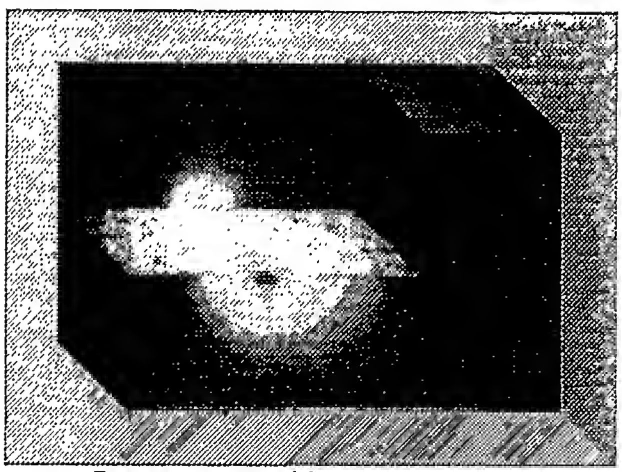
Log of tracer concentration at t = 5 minues
Max =-0.693, Min = -6



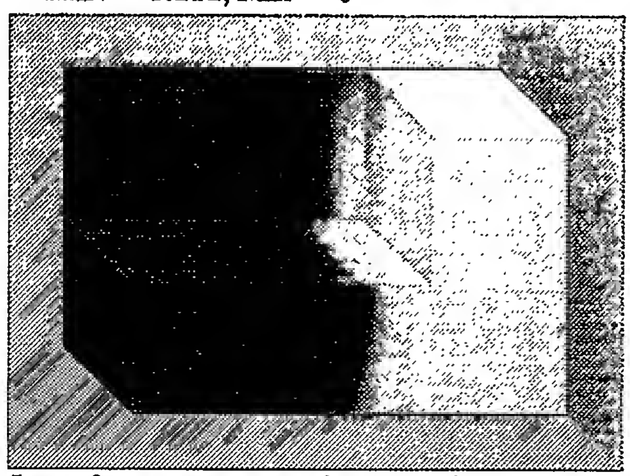
Pressure at t = 1 hour
Max = 2.175b, Min = 1.074b



Log of tracer concentration at t = 1 hour
Max = -1.212, Min = -6



Pressure at t = 4 hours
Max = 1.157b, Min = 1.007b



Log of tracer concentration at t = 4 hours
Max = -1.363, Min = -6

Figure 27. Three-dimensional color contours for the untamped case with 2 hours of pre-pressurization (Mod. 10). See Fig. 2 for dimensions and location of containment features. (Note all pressure contours are scaled to max = 0 and min = -6.)

SUMMARY

The three-dimensional porous flow calculations presented in this paper tend to support the idea that contamination of the drift complex can be avoided by proper spacing of the cavity (or zero room) from the drift and injecting air into some portion of the drift region. We have tried to provide a wide variety of simulations so that many different kinds of effects could be studied. The distance from the drift to the cavity was only 20 m, but this provides the opportunity to see how the pressure fields interact. Increasing the distance to the cavity will always improve the prospects for keeping the drift clean. It is encouraging that we were able to keep the drift clean (given the assumptions about the material properties, boundary conditions and isothermal nature of the porous flow process in this context) by utilizing injection rates that should be reasonably achievable in the field. The injection rate of 4000 cfm over the length of 68 m, translates into a pumping rate of 0.028 cubic meters per second per meter length of drift. The impermeable doors that were assumed to block off flow in a certain section of the drift, should also be able to be constructed in the field. They only need to withstand moderate pressures, generated over the course of several minutes. Also, several doors could be installed in series, thereby gradually increasing the ability to maintain a certain pressure level. These doors are visualized to be portable and relatively inexpensive.

The general conclusion from these calculations, is that the LYNER concept is viable from a porous flow point of view. Engineering solutions can be implemented to mitigate the hazard of drift contamination, given the assumptions imposed in this preliminary study. As material properties become characterized and the underground design proceeds, it is strongly recommended that new three-dimensional calculations be performed to determine how the porous flow hazard might be affected.

Acknowledgment

This work was performed under the auspices of the U. S. Department of Energy.

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CONTAINMENT RELATED PHENOMENOLOGY FROM CHEMICAL KILOTON

by
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ABSTRACT

CHEMICAL KILOTON is a DoE/LLNL verification/non-proliferation test located in zeolitized, saturated tuff in the U12n.25 drift near MISTY ECHO, MINERAL QUARRY, and HUNTERS TROPHY. The source consists of 2.9 million lb.. (approx. 1315 metric tons) of a 50/50 ANFO/emulsion mix (10^{12} calorie energy release) in a 50 ft. diameter by 17 ft. deep "tuna can" shaped cavity (volume of approx. 980 m^3). This paper summarizes the results of a series of one- and two-dimensional calculations, which were made for DNA, of the ground motions, residual stresses, and potential for hydrofracture into an open alcove located only 52 m from the cavity wall. The numerical predictions, made using tuff material properties from the nearby MISTY ECHO event, are to be compared to anticipated SNL measurements in order to validate the computational damage models for tuff used for simulations of nuclear events. These SNL measurements to late times of both radial and hoop stress components (at peak stress levels of 0.5-1.0 kb) are intended to provide direct evidence of residual stresses in this tuff.

The HE calculations, made using the JWL equation of state to describe the detonation, give peak stresses (at the 1 kb peak stress location), peak displacements, and far field ground motions much larger than calculated for a 1.0 kt nuclear source, using the same tuff model and material properties. Calculations for the nuclear source are in much better agreement with an empirical fit by Bass of the peak stress measurements *versus* scaled range from all UGT events in tuff. The approximately 50-100 percent more efficient coupling of this HE source is shown to result from the relatively large gamma (ratio of specific heats) of its detonation products as compared to the much lower gamma of the expanded gases (tuff and steam) in the nuclear cavity.

1. INTRODUCTION

CHEMICAL KILOTON was to be executed in late September of 1993 in zeolitized, saturated tuff in the U12n.25 drift near MISTY ECHO, MINERAL QUARRY, and HUNTERS TROPHY. The explosive source consisted of 2.9 million lb (approx. 1315 metric tons) of a 50/50 ANFO/emulsion mix (10^{12} calorie energy release) blasting agent which almost completely filled a 50 ft. diameter by 17 ft. deep "tuna can" shaped cavity

*Consultant

(volume of approx. 980 m^3). Detonation was to be initiated simultaneously at three separate locations along the vertical axis of symmetry of this cavity resulting in an HE detonation wave and ground motions in the surrounding tuff which may be modeled using axisymmetric two-dimensional cylindrical geometry. At distances large compared to the cavity dimensions, a spherically symmetric description of the resulting ground motions is considered adequate for the verification/non-proliferation purposes of this experiment, *i.e.*, to directly compare seismic ground motions from HE and nuclear sources.

This paper summarizes the results of a series of one- and two-dimensional pre-shot numerical simulations of the HE detonation and nonlinear tuff ground motions, which were made for DNA to study several aspects of the containment related phenomenology from this HE event. The one-dimensional spherically symmetric calculations, which are discussed first, investigate the sensitivity of the ground motions, cavity conditions, and residual stress fields both to variations in the parameters of the JWL equation of state (Ref. 1) which characterize the explosive and to the choice of constitutive model for explosively induced damage (reduction in strength) of the surrounding tuff. Two tuff damage models, the "old" volumetric strain-based model and the "new" shear strain-based model which more severely damages the rock, are described briefly in Section 2 (see Ref. 2 and 3 for a detailed description of the damage models). The tuff volumetric response and the virgin failure surface for both damage models used in the calculations were taken directly from the description of the WP region of the nearby MISTY ECHO event given by Rimer, Nilson, and Halda (Ref. 4).

In Section 3, comparisons are made between the peak stresses and displacements obtained from the HE calculations, peaks obtained from similar calculations for a 1.0 kt nuclear source, made using the same tuff material properties, and empirical fits by Bass (Ref. 5) to measurements on a number of nuclear events in similar tuffs. The numerical predictions give peak stresses and ground motions for the HE source which are much higher at any given range than those obtained from either the calculations for the nuclear source or the empirical data fits. The peaks calculated for the CHEMICAL KILOTON HE source are shown to be in much better agreement, however, with data from the earlier smaller scale SNL ONETON HE event (Ref. 6) in a G-tunnel tuff of somewhat higher strength. For the nuclear source, peak displacements calculated using the new damage model are shown to be in better agreement with the empirical data fit than those with the older damage model.

The axisymmetric two-dimensional simulations of the "tuna can" shaped explosive charge geometry, some of which examine the potential for hydrofracture into an open alcove located only 52 m from the WP, are discussed in Section 4. Results of the axisymmetric ground motion calculations are compared with the results of the spherically symmetric simulations. Containment implications of the calculational results are emphasized.

Measurements to late times of ground motions and both radial and hoop stress components (at anticipated stress levels of 0.5 to 1.0 kb) were to be made by SNL for

DNA. These measurements, intended to provide direct evidence of residual stresses in tuff, when available, will also be compared to the numerical predictions and to data scaled from the earlier SNL ONETON event.

This paper concludes in Section 5 with a summary of the similarities and differences between the ground motion phenomenology from HE and nuclear sources.

2. MATERIAL MODELS

In the absence of detailed site specific material properties measurements for CHEMICAL KILOTON, the extensive tuff material properties data from the working point region of the nearby MISTY ECHO event, in the same tunnel tuff beds, were used in the calculations of this study. In fact, the WP tuff models and properties described in Reference 4 are identical to those used here for the calculations with the old damage model. The volumetric load/unload response of this saturated tuff (mass density of 1.90 gm/cm^3, gas porosity of 0.5 percent) to the peak pressure of 4 kb, at which all gas voids are assumed fully crushed, is shown in Figure 1. For pressures less than 0.1 kb, the response is assumed elastic with a constant bulk modulus of 92 kb.

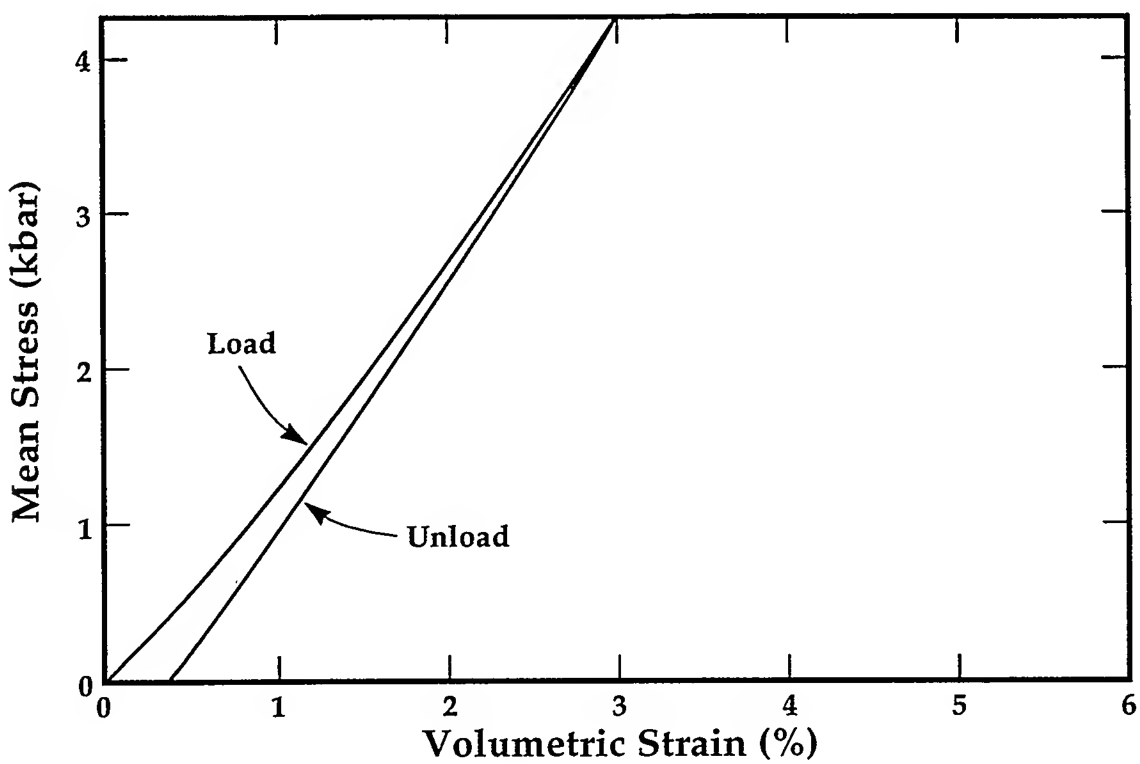


Figure 1. Mean stress versus volumetric strain on load/unload to 4 kb.

A modified elastic/plastic model with material damage is used to describe the deviatoric response of the tuff. Hooke's Law is used, with a constant shear modulus of 35 kb, until stress differences are greater than those on the failure surface. Calculated

stress differences which exceed this failure surface are reduced, using the nondilatant radial return algorithm.

As discussed in Reference 2 and 3, the failure surface is obtained by interpolation between a virgin and a damaged failure surface. For the old damage model, quasistatic tests, in which core samples are damaged in the laboratory, are used to determine the damaged failure surface. For the new damage model, however, the damaged surface is based on the much lower strengths measured in the laboratory on core samples obtained post-shot from near nuclear events. Figure 2 shows the damaged failure surfaces used here for the two damage models along with stress differences *versus* mean stress during uniaxial strain loading to 4 kb.

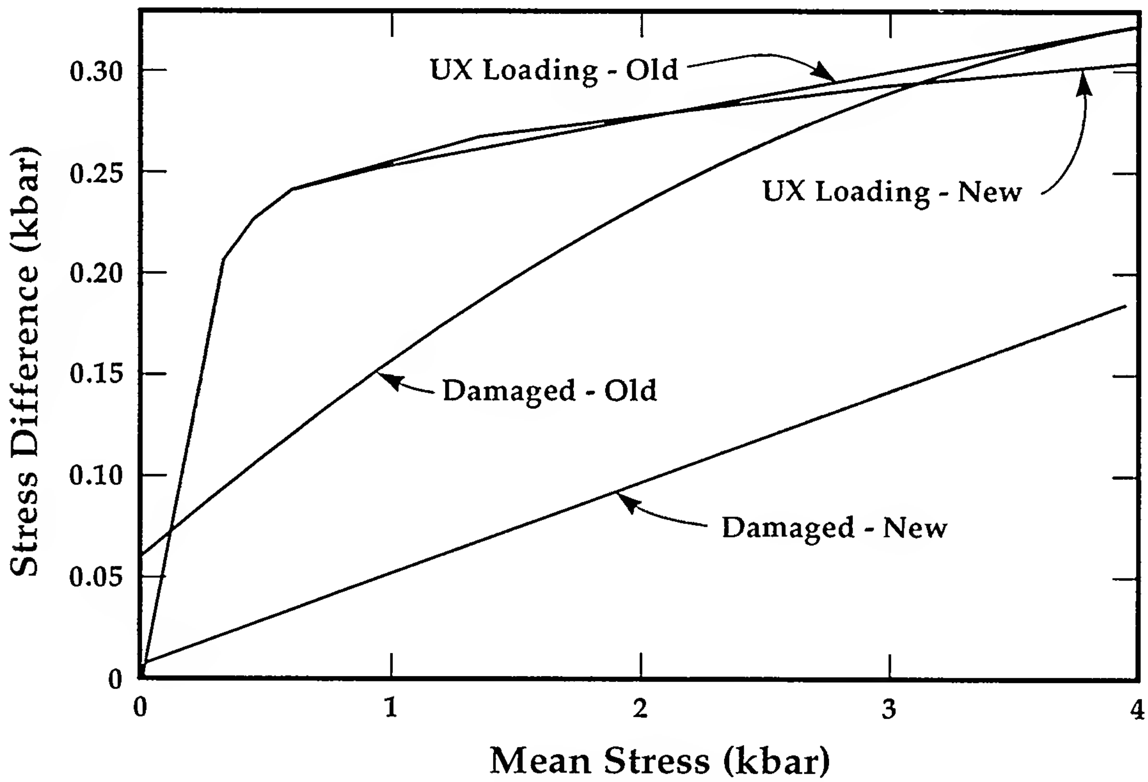


Figure 2. Uniaxial strain loading responses and fully damaged failure surfaces for old and new damage models.

For the old damage model, interpolations between the virgin failure surface (given by the uniaxial strain response after shear failure) and the laboratory damaged surface are based on the maximum volumetric strain (or equivalently, the mean stress) experienced by a tuff element during unload from stresses above 1 kb. A 4 kb mean stress is required to fully damage the tuff in the model used here. Since the lower damaged strengths for the new model, at the same mean stress, appear to result from the much larger shear strains during the *insitu* divergent outward ground motions near the cavity, interpolation between the virgin and damaged failure surfaces is based on the maximum shear strain. An 8.0 percent shear strain is assumed in these calculations

to fully damage the tuff. At the considerably lower shear strains achieved in the laboratory, the new damage model also reproduces the results of the laboratory damage tests.

For the calculations with the nuclear source, the cavity is assumed initially to contain approximately 70 metric tons of normal bulk density vaporized tuff (a cavity radius of 2.06 $m/kt^{1/3}$). Earlier parameter studies (Ref. 7) have shown that initialization with a smaller cavity containing 17.5 tons/kt of tuff (a cavity radius of 1.30 $m/kt^{1/3}$) gives calculational results of interest here which are almost identical. The vaporized tuff cavity pressure is calculated using a γ-law gas equation of state with a variable γ, a function of density and internal energy, obtained from isentropic releases from the CHEST equation of state for tuff (Ref. 8). CHEST is a tabular mixture under conditions of chemical equilibrium of all of the major components of a saturated tunnel tuff. For the cavity conditions of our calculations, the variable γ initially is approximately 1.57, but rapidly decreases to below 1.1 during the cavity expansion.

For the seven HE detonation calculations of this study, the Jones-Wilkins-Lee (JWL) equation of state (Ref. 1) is used to describe the pressure-volume-energy behavior of the explosive detonation products of the 50/50 ANFO/emulsion mix. The JWL equation is:

$$P = A\left(1 - \frac{w}{R_1 V}\right)e^{-R_1 V} + B\left(1 - \frac{w}{R_2 V}\right)e^{-R_2 V} + \frac{wE}{V}$$

where V is the relative volume (specific volume of detonation products)/(specific volume of undetonated explosive), P is the pressure, E is the internal energy/initial volume, and A, B, R1, R2, and w are material constants with values specified for any given explosive. For our finite difference codes, which use specific internal energy, ε, and mass density, ρ, as variables, E/V is replaced in the last term of this equation by $\varepsilon\rho$. This last term, which soon becomes the dominant term of the JWL equation as the detonation products expand, is simply an ideal gas gamma-law equation, with w equal to γ-1.

Reference 1 contains a listing of the material constants of the JWL equation for many well characterized explosives. Also given are the total available energy, E_o, (or ε_o), the initial density, ρ_o, and the parameters used to characterize the Chapman-Jouget (C-J) state of the explosive, the C-J pressure, P_{cj}, the speed of a detonation wave, D, and the quantity, Γ, the reduced compression ratio, used to define the C-J density, ρ_c. From these quantities, the particle velocity, U_c, and specific internal energy, ε_c, at the C-J state may be obtained from the following set of equations:

$$\rho_c = \rho_o(\Gamma+1)/\Gamma$$

$$V_c = \rho_o/\rho_c$$

$$U_c = D(1-V_c)$$

$$\varepsilon_c = \varepsilon_o + 0.5P_{cj}(1-V_c)/\rho_o.$$

The "Burn Fraction" technique was used in this study to simulate the explosive detonation, with the burn fraction, BF, defined as $(1-V)/(1-V_C)$. BF, which is used to multiply the HE pressure calculated from the JWL equation, can be shown to have a magnitude less than 1.0 until the HE has fully detonated, *i.e.*, has reached the C-J state. In this study, the HE was discretized using 128 finite difference zones and a detonation wave initiated in the first 20 of these zones. The last of these 20 grid zones was initially set at the C-J state, the first zone at zero particle velocity and ambient HE conditions, with values of U, ε and ρ in the other 18 zones obtained from linear interpolation with range. (BF is also set to 1.0 in the first 20 zones.) In each subsequent zone in the propagating detonation wave, the calculated BF is first set to 1.0 whenever it is greater than 0.9, before multiplying the pressure calculated using the JWL equation.

This procedure has been found to result in the desired C-J state in each grid zone once the detonation wave has propagated roughly 30 zones from the initialization region. Simpler procedures which less accurately approximate the detonation wave are usually used for two-dimensional calculations for which fewer grid zones are available to discretize the HE. However, in this study, all of the two-dimensional (and a few of the one-dimensional) numerical simulations were made using the JWL isentrope equation to calculate the cavity pressure, which was assumed uniform throughout the HE cavity at any time step of the calculations. The equation for P as a function of V at constant entropy, *i.e.*, the JWL isentrope, is

$$P_S = Ae^{-R_1 V} + Be^{-R_2 V} + CV^{-(w+1)}$$

where C is a material constant. Using one-dimensional calculations, it will be shown later in this paper that the tuff ground motions (except very near the cavity wall), the late-time HE cavity radius and pressure, and the tuff residual stresses calculated using the JWL isentrope are very similar to those obtained from detailed calculations of the propagation of the HE detonation wave.

The propagation of the detonation wave was explicitly modeled in a series of seven spherically symmetric ground motion calculations. Three different sets of parameters for the JWL equation of state, each based on the best information available for the ANFO/emulsion mix at a given time during this study, were used in the calculations (See Table 1). Runs 1, 2, 3, and 5 were based on JWL parameters obtained by LLNL for a different explosive mix. Some of these parameters were then modified to conform to HE parameters obtained from Olsen (Ref. 9). Parameters for a 70/30 ANFO/emulsion mix, notably a much higher P_{cj}, obtained from the supplier, IRECO Inc., were used for Run 4. IRECO information for the 50/50 mix finally chosen for CHEMICAL KILOTON and JWL parameters from Souers (Ref. 10) were used for detonation Runs 6 and 7 and the subsequent JWL isentrope runs.

Table 2 shows some results of the seven detonation calculations which illustrate the sensitivities of the cavity conditions, motions and residual stresses in the tuff to the

Table 1. JWL Equation of State parameters for ANFO Emulsion mix.

Parameter Set	Preliminary Run 1,2,3,5	70/30 Mix Run 4	50/50 Mix Run 6,7,8a,9a#, Two-Dim. Calc.
ρ_0 (gm/cm^3)	1.28	1.26	1.34
ε_0 (10^9 erg/gm)	36.48	36.70	32.23
D (cm/μsec)	0.5624	0.6462	0.6849
CJ-Conditions			
P (kb)	90	130	144
ρ (gm/cm^3)	1.7138	1.687	1.7452
ε (10^9 erg/gm)	45.38	49.80	44.71
U (cm/μsec)	0.1424	0.1636	0.1590
Γ	2.9508	2.9508	3.307
JWL Coeff. *			
A (kb)	2319.4	3895.77	4711.22
B (kb)	96.535	96.535	120.0
w	0.30+	0.22	0.30

8a,9a are JWL Isentrope runs with C = 4.49 kb.
* R_1 = 5.002 and R_2 = 1.55 for all runs.
\+ w = 0.22 for Run 1 only.

JWL parameters and to the choice of computational damage model for the tuff. Runs 1, 2, 4, and 6 were made with the old volumetric strain-based damage model and Runs 3, 5, and 7 with the new shear strain-based damage model which more severely weakens the tuff surrounding the HE cavities. The magnitude and location of the peak residual hoop stress and the amount of cavity overshoot are all far more sensitive to the choice of damage model than to the JWL variations in the calculations. However, the calculations do suggest the following effects of variations in P_{cj} and w:

- Higher P_{cj} results in increased peak stress in the tuff at a given range near the cavity wall. (The 20 kb peak stress contour is further from the wall of the spherized cavity volume.)

- Higher P_{cj} or higher w both result in enhanced cavity overshoot and lower cavity pressure. (Residual hoop stress magnitudes vary directly with cavity pressure.)

Table 2. Sensitivity of 1 kt detonation runs to HE parameters and choice of damage model.

Run #	1	2	4	6	3	5	7
JWL Coef.	PCJ=90* w=0.22	PCJ=90 w=0.30	PCJ=130 w=0.22	PCJ=144 w=0.30	PCJ=90 w=0.30	PCJ=90 w=0.30	PCJ=144 w=0.30
Damage Model	old	old	old	old	new#	new	new
20 kb loc. from wall (m)	5.0	5.0	5.1	5.9	5.0	5.0	5.9
<u>Cavity</u>							
Pressure (bars)	128.4	122.2	119.9	90.45	123.5	123.8	104.8
Radius (m)	16.95	17.41	16.64	16.63	17.42	17.42	16.14
Over-shoot (m)	0.87	1.06	0.94	1.26	3.05	3.30	3.77
<u>Residual</u>							
Hoop Stress (bars)	301	293	291	264	194	188	176
Radial Range (m)	53	55	55	61	90	93	94.5

* PCJ=Chapman-Jouget Pressure (kb), w= γ-1 of expanded products.
# Poorer choice of damaged failure surface.

Since the JWL parameters used in Runs 6 and 7 best represent the response of the 50/50 ANFO/emulsion blasting agent to be used on CHEMICAL KILOTON, these parameters will be used in the predictive calculations of this study.

3. HE-Nuclear Equivalence

This section summarizes the results of the site specific spherically symmetric ground motion calculations. Emphasis is placed on comparisons between the peak ground motions, cavity conditions, and residual stresses calculated for the 1.0 kt HE source and those calculated for a 1.0 kt nuclear source. The calculated peak stresses and peak displacements *versus* range will also be compared with empirical fits by Bass (Ref.

5) to the ground motion data from nuclear events in tuff, and to the data from the smaller scale SNL ONETON TNT event in G-tunnel tuff (Ref. 6).

Table 3 summarizes some containment related results for the seven calculations to be compared. These calculations include detonation Runs 6 and 7 which were discussed earlier, Runs 8a and 9a which model the HE mix using the JWL Isentrope equation, Runs 10 and 12 which model a nuclear source as tuff using the variable γ fit to

Table 3. Comparisons between results of spherically symmetric calculations for HE detonation, HE isentrope, and nuclear sources.

Run #	6	7	8a	9a	10	11	12
Source	HE DET.	HE DET.	HE ISEN.	HE ISEN.	NE	γ=1.3	NE
Damage Model	old	new	old	new	old	old	new
Cavity							
Pressure (bars)	90.45	104.8	100.0	113.3	106.8	102.9	116.4
Radius (m)	16.63	16.14	16.28	15.77	14.09	16.49	13.76
Overshoot (m)	1.26	3.77	1.17	3.60	0.96	1.14	3.21
Energy (%)	13.9	14.6	27.0	27.5	34.9	15.3	35.1
Rebound Time (msec)	87.9	114.2	85.0	109.2	75.5	88.5	99.0
Residual							
Peak Hoop Stress (bars)	264	176	269	173	279	277	176
Radial Range (m)	61	94	59	91	50	59	80
Crossover Range (m)	76	110	72	107	62	73	93
Elastic							
Yield Radius (m)	127	140	123	135	108	126	120
Elastic Radius (m)	349	349	341	341	294	345	299
RDP (m^3)	530	410	500	380	360	580	290

CHEST discussed earlier, and Run 11 which models the nuclear source as tuff with a constant γ of 1.3. Each of the three source descriptions, excluding Run 11, was used in two calculations, with the old and the new tuff damage models, in an attempt to bound the predicted phenomenology. The nuclear sources were initialized with a cavity radius of 2.06 m, considerably smaller than the 6.139 m spherized radius required for the 1.0 kt available energy of the chemical explosive.

Runs 8a and 9a were executed primarily for direct comparison with the cylindrical two-dimensional calculations to be discussed later. For these runs, the cavity pressure is applied as a pressure boundary condition on the wall of the expanding cavity with the assumed uniform cavity conditions, initial density of 1.34 gm/cm^3 and available energy of 4.471×10^{10} ergs/gm, giving an initial cavity pressure of 61.64 kb from the JWL Isentrope equation, over a factor of two lower than the detonation pressure of 144 kb. At late time, these calculations give twice as much energy remaining inside the cavity (but only 10 percent higher late time cavity pressure) than obtained from the comparable detonation calculation. While the late time cavity conditions calculated using the JWL Isentrope equation appear to be thermodynamically consistent with the detonation products from the ANFO/emulsion mix, the late time cavity conditions obtained from the detonation calculations, at least for the JWL material constants used here, appear to be thermodynamically inconsistent with these detonation products. Fortunately, the differences in calculated results due to the two HE treatments are insignificant outside of the immediate vicinity of the cavity.

The results of the seven calculations shown in Table 3 are organized into three groups, cavity conditions, residual stresses, and far field (elastic). In the cavity region, the lower initial cavity pressure for the isentrope runs results in lower peak stresses in the tuff near the cavity, less cavity expansion, less plastic work, and therefore a slightly smaller cavity overshoot than calculated in the corresponding detonation run. Rebound time, residual stress magnitudes and locations, and far field results are only changed slightly.

The choice of damage model for the tuff results in much larger changes in cavity overshoot, rebound time, residual stresses and yield radius (similar to those described in References 2 and 3). As shown in Table 3, all of the calculations with the new damage model gave somewhat smaller cavity radii than the comparable calculations made with the old damage model. Since the new damage model implies a much weaker tuff surrounding the cavity after the motion-induced damage, the smaller calculated cavity radii appear to be against our intuition based on static assumptions. However, the calculations show very different dynamics for the new damage model, with both enhanced outward and rebound displacements. Thus, the qualitative difference in final cavity radius, which depends upon the damage parameters used in the model in these very nonlinear calculations, may not be intuitively predictable.

For all calculations, overshoot, the difference between maximum and final displacement, is greatest at the cavity wall and decreases with increased range. (The

relative overshoot, defined here as the overshoot/final displacement, increases with increased range out to the elastic radius.) The static value of the reduced displacement potential (RDP), proportional to the final displacement, is smaller for the new damage model, but the overshoot is much larger. Particularly for the new damage model, the static RDP is probably not a good measure of the seismic motion for the peaked spectra resulting from these large overshoots.

Figure 3 shows the radial and hoop residual stress components *versus* radial distance from the center of the explosive charge for Runs 6 and 7. As expected, use of the new damage model, which drastically weakens the rock surrounding the cavity, results in lower peak residual stresses and moves these peaks out to significantly larger ranges. (In Table 3, the residual stress crossover range is the radial location of the peak radial residual stress.) Comparisons to be made with the SNL measurements, when available, may help to establish which of the two damage models provides a more realistic description of the tuff strength behavior.

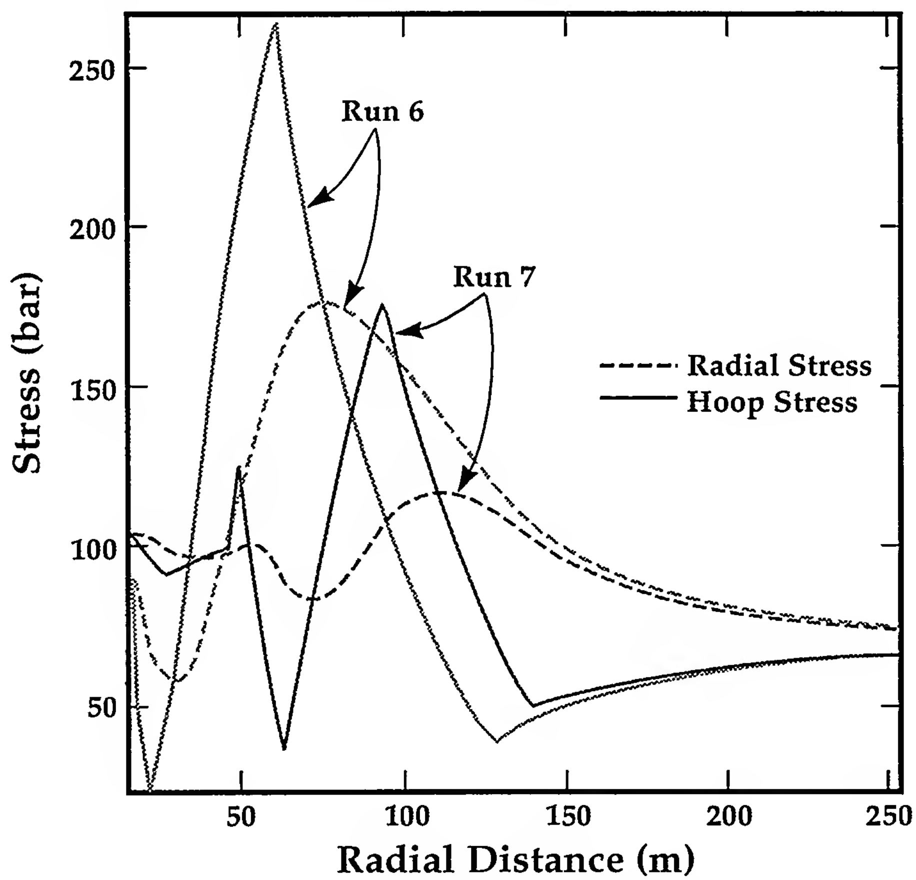


Figure 3. Radial and hoop components of residual stress from detonation Run 6, for the old damage model, and Run 7, for the new damage model.

The results shown in Table 3 for the nuclear sources, Runs 10 and 12, are somewhat different from those for the comparable HE sources, Runs 6 and 7 respectively. The nuclear source results in about 15 percent decreases in cavity radius, cavity over-

shoot, rebound time, yield radius, and elastic radius. Similar decreases are calculated in the ranges to residual stress peaks. The 30-32 percent calculated decreases in static RDP are in reasonable agreement with the 34 percent lower device yield required, using cube root of yield scaling, to reduce range-to-effects by 15 percent. These theoretical results suggest that the nuclear yield is 30-34 percent less efficient than the HE or equivalently, that the chemical explosive couples approximately 50 percent more efficiently to the ground than does a nuclear source of the same yield. (The different initial source radii for HE and NE did not explicitly enter this yield scaling analysis.)

The JWL equation of state used for detonation Run 6 degenerates to an ideal gas equation of state with a constant gamma of 1.3 once the HE cavity gases expand (V becomes large). Therefore, the results of Run 11, made using an ideal gas equation of state for the tuff cavity gases with the same gamma, provide a possible explanation for the very different coupling between HE and nuclear sources. Run 11, with an initial cavity radius of 2.06 m, gives peak stresses and peak displacements, cavity radius, cavity energy, rebound time, yield and elastic radius, and RDP which are all very similar to detonation Run 6, for the much larger initial HE cavity. Thus, the more realistic variable cavity gamma used in Run 10 (which gives a gamma below 1.1 for the expanded cavity gases) must be the explanation for the much less efficient coupling of the nuclear source to the surrounding rock.

Although it has not been proven that the cavity model used in Runs 10 and 12 is appropriate for a nuclear event, comparisons with empirical fits to peak stress data by Bass (Ref. 5) strongly suggest that this cavity treatment is superior to the use of a constant gamma. Figure 4 shows peak stress versus scaled range from the Bass fits to data from nuclear events and to data from the ONETON TNT event. (The individual ONETON data points are also shown.) The fit to peak stress from nuclear events in tuff is given by

$$P = 861.7\ R^{-1.789}\ \text{(kb)}$$

and the fit to ONETON by

$$P = 1526\ R^{-1.839}\ \text{(kb)}$$

where R is the radial range, in meters, scaled to 1.0 kt using cube root of yield scaling. The data fits are applicable only to stress levels above 0.3-0.5 kb.

Using the empirical fit to nuclear data, a nuclear yield of 1.8-2.0 kt is required to fit the ONETON peak stress data, scaled to 1.0 kt. (The difference in slopes between the two fits gives slightly different HE-NE equivalence factors at different scaled ranges.) Caution should be exercised in generalizing this result since this TNT event was in a tuff which had a relatively low gas porosity (0.7 percent) compared with the nuclear events which were in tuff with 1-2 percent gas porosity on average. The lower gas porosity results in somewhat larger peak stresses in ground motion calculations,

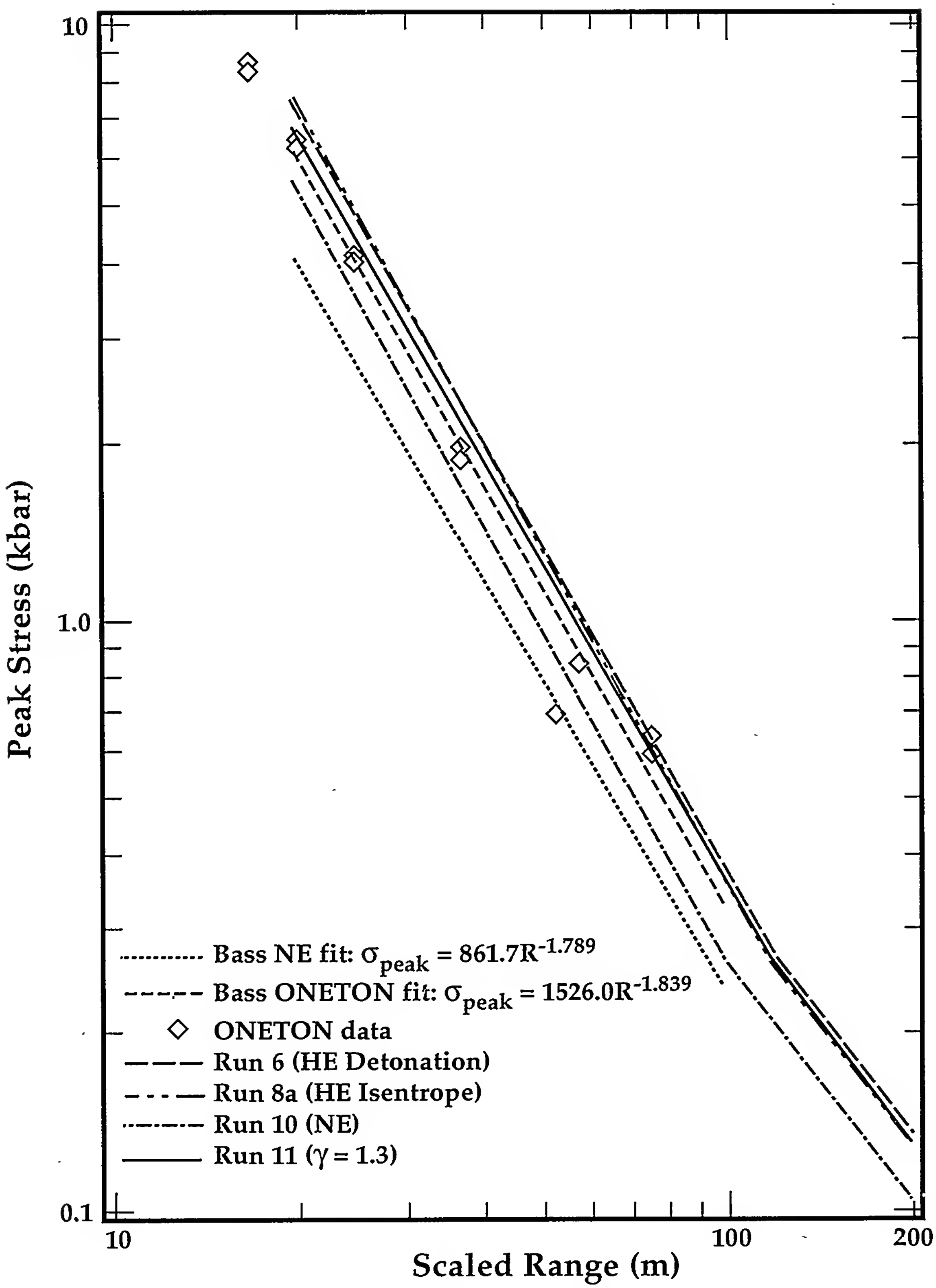


Figure 4. Peak stress vs. scaled range for nuclear and HE events.

enough to remove one quarter to one-half of the difference between the peak stresses in the two data sets. (Although some of the other phenomenology in this relatively small HE event might not cube-root-of-yield scale up to 1.0 kt due to rock size or strain rate effects, peak stress should be scaleable.)

Also shown in Figure 4 are the results of the four calculations of Table 3 which were made using the old damage model. The comparable calculations with the new damage model, at the smaller ranges of Figure 4, give very similar peak stresses to the calculations shown. At larger ranges, the new damage model gives peak stresses which are larger by only a few percent.

Peak stresses calculated for the best nuclear source, Run 10, are considerably larger than given by the empirical fit to NE data (but do lie within the scatter of the nuclear data which were used in the fit). Most of the difference can be attributed to the 0.5 percent gas porosity used in our site specific calculations. This area of N-tunnel has been observed to more strongly couple nuclear events than other Rainier Mesa sites. The use of the constant gamma source in Run 11 greatly increases peak stresses, further increasing the disagreement with the empirical fit. Run 11 gives peak stresses in excellent agreement with the calculations for the CHEMICAL KILOTON source.

Comparisons between the peak stresses from detonation Run 6 and NE Run 10 give an HE-NE equivalence factor which varies between 1.64 (at 5.0 kb) and 1.85 (at 0.3 kb). These are on average slightly smaller than the factor of 1.8-2.0, obtained from the NE and ONETON empirical peak stress data (for a different site and HE), but are larger than the 1.5 factor obtained above from the RDP values and other results in Table 3.

Figure 5 shows peak displacement *versus* scaled range for some of the site specific NE and ANFO/emulsion calculations discussed earlier. Also included are the Bass empirical fit (Ref. 5) to peak displacements from nuclear events in tuff and the four data points from ONETON, scaled to 1.0 kt. The Bass fit to peak displacement is given by

$$\text{Displ.} = 20{,}300\ Y^{1/3}\ R^{-1.584}\ (\text{cm})$$

where Y is the device yield, in kilotons. Most of the NE data for this fit was taken from beyond scaled ranges of 90 m (the fit is arbitrarily cut off inside of a scaled range of 40 m in the plot).

Due to the scatter of the sparse displacement measurements for ONETON, no empirical data fit was given by Bass (Ref. 5) although three of the four ONETON data points shown in Figure 5 appear to be very consistent. These HE data points clearly trend well above the Bass empirical fit to peak displacements from tuff nuclear events (at least for scaled ranges less than 80 m). However, we did not use these data to calculate an HE-NE equivalence factor because of concerns regarding the ONETON source size and the relevant site properties.

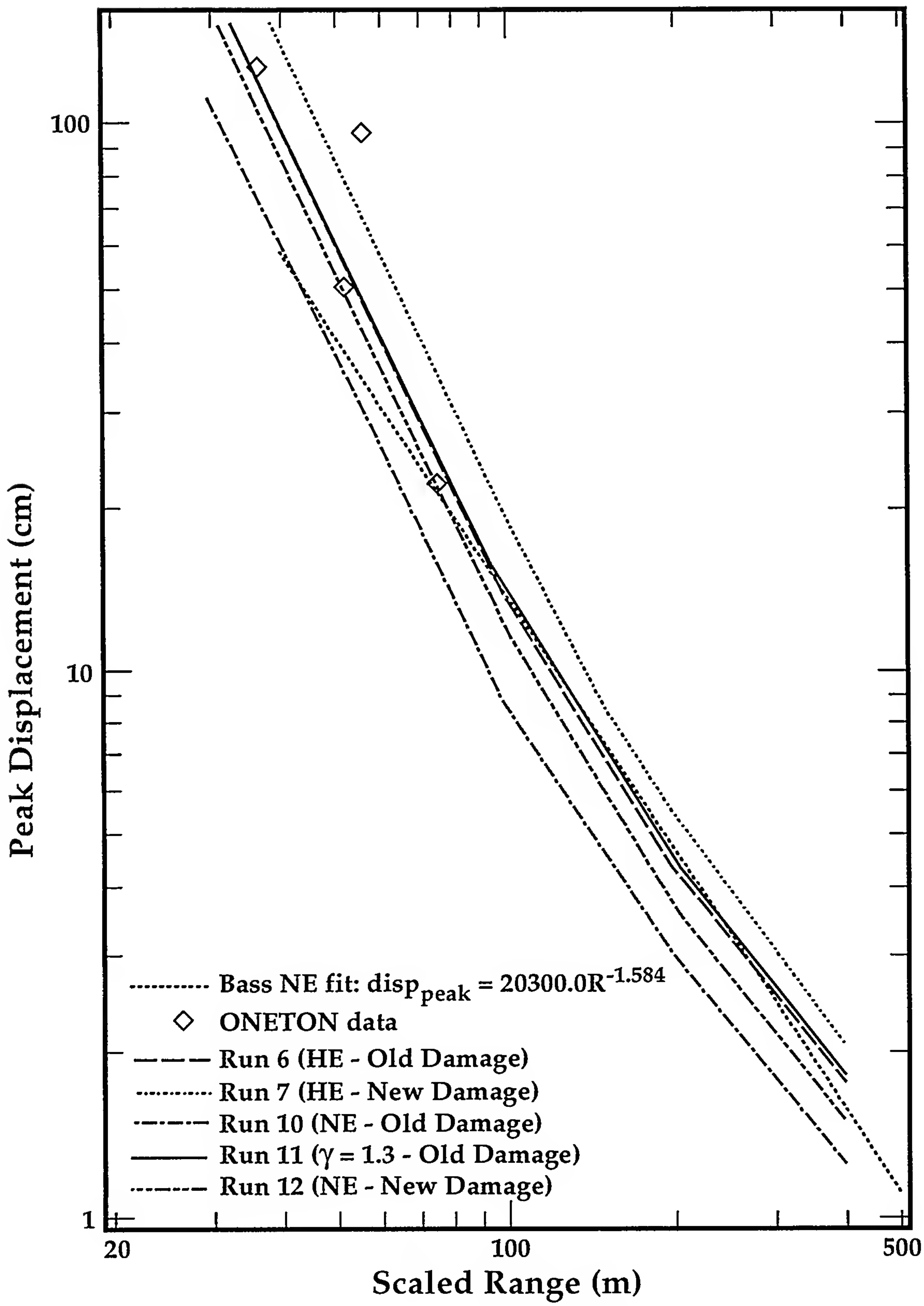


Figure 5. Scaled peak displacement versus scaled range for nuclear and HE events.

Pre-shot predictive calculations for ONETON (see Ref. 11 and 12), made using measured site properties, clearly showed that a relatively high strength site model (and perhaps even the inclusion of strain rate dependent strength increases for the smaller source) was required to simulate the shot data. Thus, the scaled displacements measured on ONETON would be expected to be considerably lower than for an HE event on an average tuff site or the weaker CHEMICAL KILOTON site. (The average measured cavity radius from ONETON, 13.4 $m/kt^{1/3}$, also consistent with a strong site, is considerably smaller than given in Table 3, for the weak HE site calculated here.)

Nevertheless, the displacement data from ONETON appear to have been used by Bass (Ref. 5) to define HE and NE sources as roughly equivalent. Bass compared peak stresses and peak displacements from a particular nuclear event (MISTY RAIN) with data from ONETON. He argued that MISTY RAIN, which clearly coupled more strongly than given by his fits to all events, had a low gas porosity, more in line with ONETON site properties, and thus should provide more relevant comparisons. This argument neglects the crucial importance of the rock strength in reducing the displacements and far field ground motions in the ONETON tuff and in increasing the motions and displacements in the relatively weak tuff at the MISTY RAIN site. Thus the fact that the displacement data for ONETON and MISTY RAIN imply an HE-NE equivalence of approximately 1.0 is fortuitous and incorrect.

Beyond a scaled range of about 70 m, the Bass empirical fit is in much better agreement with calculated displacements from NE Run 12, made with the new damage model, than with Run 10, which used the old damage model. For either damage model, the calculations give much larger displacements for the HE sources than for the comparable NE source. For peak displacements between 10 and 100 cm, these comparisons give HE-NE equivalence factors of 1.9-2.2, with the larger values for the new damage model.

In summary, the results of the site specific calculations indicate that CHEMICAL KILOTON should couple from 50 to 100 percent stronger than a nuclear event at the same site, depending of course on the ground motion parameter used to define this coupling.

4. TWO-DIMENSIONAL CALCULATIONS

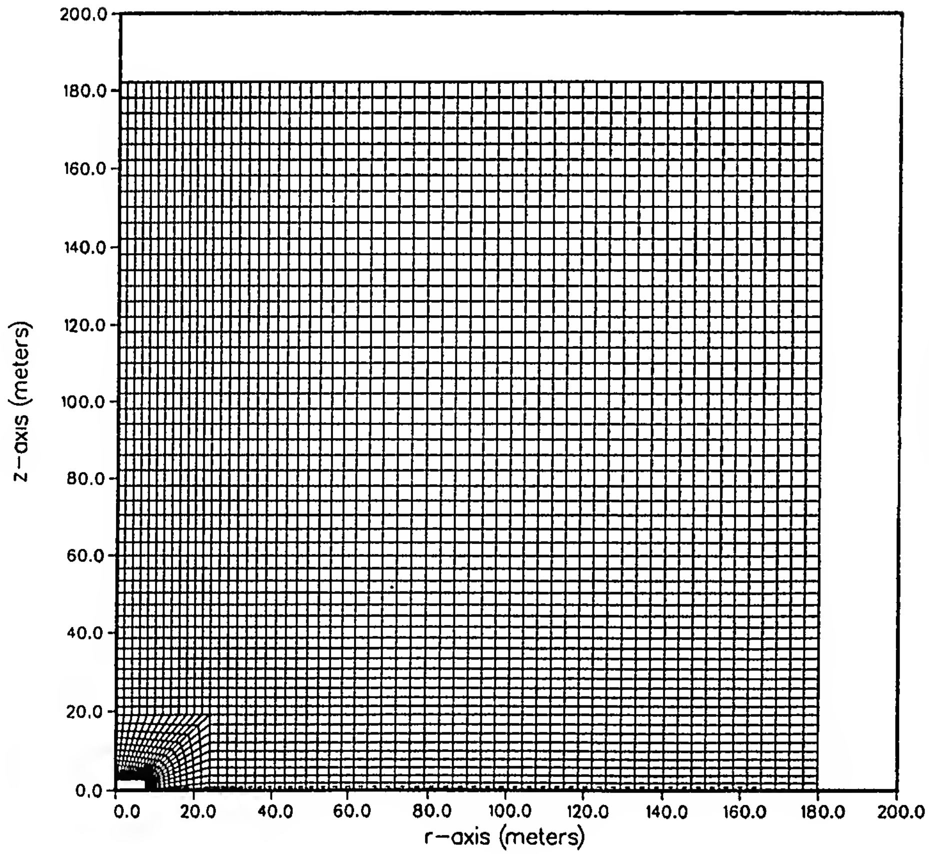
A series of Lagrangian axisymmetric two-dimensional ground motion calculations were made for the CHEMICAL KILOTON "tuna can" shaped explosive charge geometry. All calculations were made using the F-Cubed (Fast Fluid Fracture) code, containing dynamic hydrofracture algorithms (Ref. 13), developed specifically by S-Cubed to model the interactions between hydrofracture propagation and nonlinear ground motions for application to the containment of nuclear events. The calculations, labeled Chem1-Chem4, rather than modeling the detailed propagation of a detonation wave, approximated the HE source using the JWL Isentrope equation in an identical manner to the spherically symmetric one-dimensional Runs 8a and 9a discussed earlier.

This assumption of uniform cavity conditions is required by the hydrofracture algorithms which use the thermodynamic state of the cavity in computing fluid pressure and temperature distributions along the fracture at any given time. (Two-dimensional calculations by Patch (Ref. 14), using either line charge or center initiation of the HE detonation and propagation, showed only small effects of the different HE treatments on containment related results such as ground motion and residual stresses, consistent with our one-dimensional results discussed earlier.)

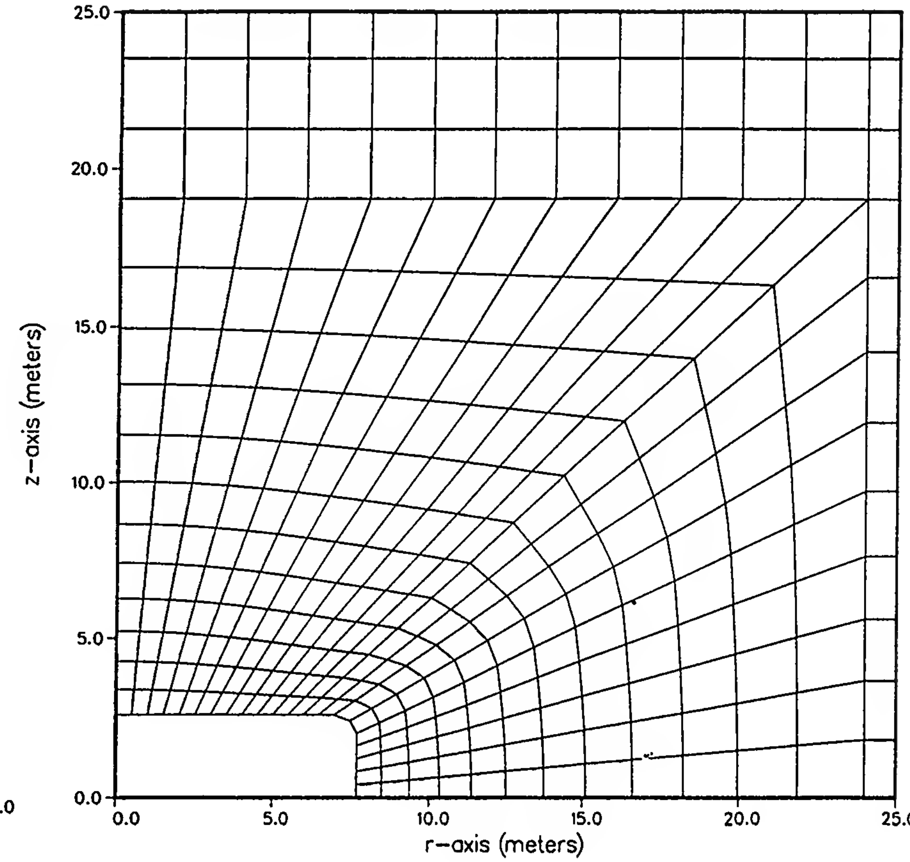
Calculations Chem1 (using the old tuff damage model) and Chem3 (using the new damage model) did not allow any hydrofracture propagation, but the corresponding calculations, Chem2 and Chem4 respectively, both assumed a single, worst case horizontal hydrofracture propagating through the tuff along the equatorial plane of the cavity. (Although F-Cubed has the capability to compute multiple fractures, these will invariably be shorter and less threatening.) An additional two-dimensional calculation, Chem5, was made, using a spherized 1.0 kt HE source and the new tuff damage model, for direct comparison with both one-dimensional Run 9a and with Chem3, for the cylindrical HE source.

These calculations all assumed a uniform tuff media of infinite extent surrounding the explosive charge, thus neglecting both the effects of the free surface (400 m above the cavity) and of a weaker, higher gas porosity (6.0 %), unzeolitized, tuff layer beginning approximately 60 m (roughly four final cavity radii) above the center of the HE cavity. The results of earlier calculations of a nuclear event in a similar geological configuration (Ref. 4) suggest that inclusion of this layering would introduce relatively small rarefaction waves at tunnel depth, thus reducing the computed residual stresses only slightly and resulting in an upward drift of the center of the final cavity of roughly 1-2 m. For the simplified geology chosen for our calculations, horizontal and vertical planes of symmetry through the cavity center (the origin in Figure 6) greatly reduce the effort required to compute out to the times of 250-350 ms necessary to obtain residual stresses.

Figure 6a shows the initial CRAM finite difference grid which extends out both radially and axial to about 180 m from the cavity for all the two-dimensional calculations. CRAM is the Lagrangian stress wave code which computes the nonlinear ground motions due to the HE explosion. It is combined in F-Cubed with a number of possible hydrofracture algorithms which compute the pressure at any given time along the assumed crack (the dashed line in the bottom row of grid zones for Chem2 and Chem4), using the crack opening displacements and cavity conditions computed in CRAM. The CRAM grid is surrounded by compatible zoning from the linear elastic, small displacement SAGE code out to radial and axial ranges of roughly 520 m. The simpler algorithms in SAGE provide a more computationally efficient way to calculate out to late times (and large distances from the source) without using an excessive number of CRAM zones.



(a)



(b)

Figure 6. Initial CRAM grid for cylindrical HE source. Figure 6b is an enlargement of the cavity region, the lower left-hand corner of 6a.

Figure 6b, an enlargement of the lower left-hand corner of Figure 6a, shows the spider web interior grid surrounding the HE cavity and transitioning into the outer, rectangular grid. Note that the corner of the cavity has been rounded slightly to accommodate the zoning. For the spherized cavity, Chem5, the interior grid zoning will be radial. Zone dimensions are smallest nearest the cavity and increase with range to match up with the more coarsely zoned outer grid. The spider web grid lines will be removed (rezoned) during the cavity expansion as the zones nearest the cavity are radially compressed. Since the zone sizes are much larger than used for the one-dimensional calculations, the very high frequency responses, *i.e.*, the calculated peak stresses and particle velocities, are expected to be somewhat reduced. However, the zoning is sufficient to adequately compute the cavity displacements and residual stresses as will be demonstrated by comparisons with the one-dimensional results given in Table 4.

Table 4 compares the results of Chem1 and Chem3, for the cylindrical HE source, with Chem5, Run 8a, and 9a, for the spherical source. The greatest differences in cavity

Table 4. Comparison between results of spherically symmetric and two-dimensional HE Isentrope Runs.

Run #	Chem1	Chem 3	Chem 5	8a	9a
Geometry	tuna can	tuna can	2D sphere	sphere	sphere
Damage Model	old	new	new	old	new
Cavity					
Pressure (bars)	108.1	117.4	117.0	100.0	113.3
Radius (m) spherized	15.89	15.56	15.57	16.28	15.77
horizontal	17.40	14.35	15.56		
upward	13.57	18.02	15.55		
Overshoot (m)	1.00	3.38	3.62	1.17	3.60
Energy (%)	28.2	28.4	28.5	27.0	27.5
Rebound Time (msec)	82.1	108.6	109.4	85.0	109.2
Residual*					
Peak Hoop Stress (bars)	273-290	181-196	179-186	269	173
Radial Range (m)	51-52.6	86.4	91.8	59	91
Yield Radius (m)	118.7	133.2	133.1	123	135

* In equatorial (horizontal) plane.

radius, overshoot, rebound time, and residual stresses were once again due to the choice of tuff damage model. For the CRAM calculations, rebound time is defined in Table 4 as the time at which the cavity volume is a maximum, while overshoot is defined as the difference between the spherized radius at this maximum volume and the final spherized radius of the cavity. Cavity radii are given in Table 4 along both axes of symmetry while yield radii and residual stresses are given in the horizontal plane of symmetry (Two values are shown where in-plane and out-of-plane hoop stress magnitudes or locations are not the same).

Figure 7 shows the final cavity shapes calculated for the initially cylindrical HE sources. Chem1, made with the old damage model, gave the expected more or less elliptical final cavity shape (at 250 ms), with the major axis in the horizontal direction, in agreement with the initial cavity shape. Chem3, however, made with the new damage model, gave a final calculated cavity shape (at 350 ms) which was qualitatively very different, with the major axis vertical. Since this result was unexpected, and not easily explainable, Chem5 was made to check out both the new damage model and the numerics in CRAM.

Figure 8 shows the shape of the Chem5 cavity at 350 ms, demonstrating that the CRAM code is capable of computing a spherical cavity expansion and large overshoot with the new damage model. The comparisons in Table 4 between Run 9a and Chem5 show good agreement between the results with the one- and two-dimensional codes. The residual stress results given in Table 4 are for a time of 1.0 sec for Run 9a while the Chem5 results were only run to 350 ms. (Stress fields and cavity radii are still changing with time at 350 ms in all calculations for the new damage model.) Peak stresses and displacements at a range of 40 m (the closest range of the expected SNL measurements) are 18 and 6 percent larger respectively for the more finely zoned Run 9a. At larger ranges, Run 9a gives peak stresses and displacements which are within 10 and 3 percent respectively of those from Chem5.

Comparisons in Table 4 between Chem5 (or Run 9a) and Chem3, the cylindrical source, show almost identical spherized cavity radii and rebound times, but show slightly different <u>average</u> cavity overshoots. However, the displacements of the cavity wall in the vertical and horizontal directions have very different magnitudes after rebound for Chem3. Figure 9 shows separately the horizontal and vertical cavity radii calculated for both Chem3 and Chem1 (and for the corresponding one-dimensional sources) *versus* time, after 100 ms. Rebound times, *i.e.*, the times at which the cavity volumes begin to decrease, are 108.6 and 82.1 ms, respectively, for Chem3 and Chem1. Until these rebound times, Chem3 and Chem1 are qualitatively very similar, with larger cavity radii, as expected, horizontally than vertically, mirroring the initial cavity shape shown earlier in Figure 6b. (The motions and overshoots for Chem1 are much smaller and relatively uninteresting.) Both calculations show the difference between horizontal and vertical cavity radius, initially about 5 m, being reduced during the outward cavity motion. Both also show radially outward motions stopping first in the horizontal direction and last in the vertical direction, as a result of the smaller impulse in the horizontal direction due to the initial shape of the HE cavity.

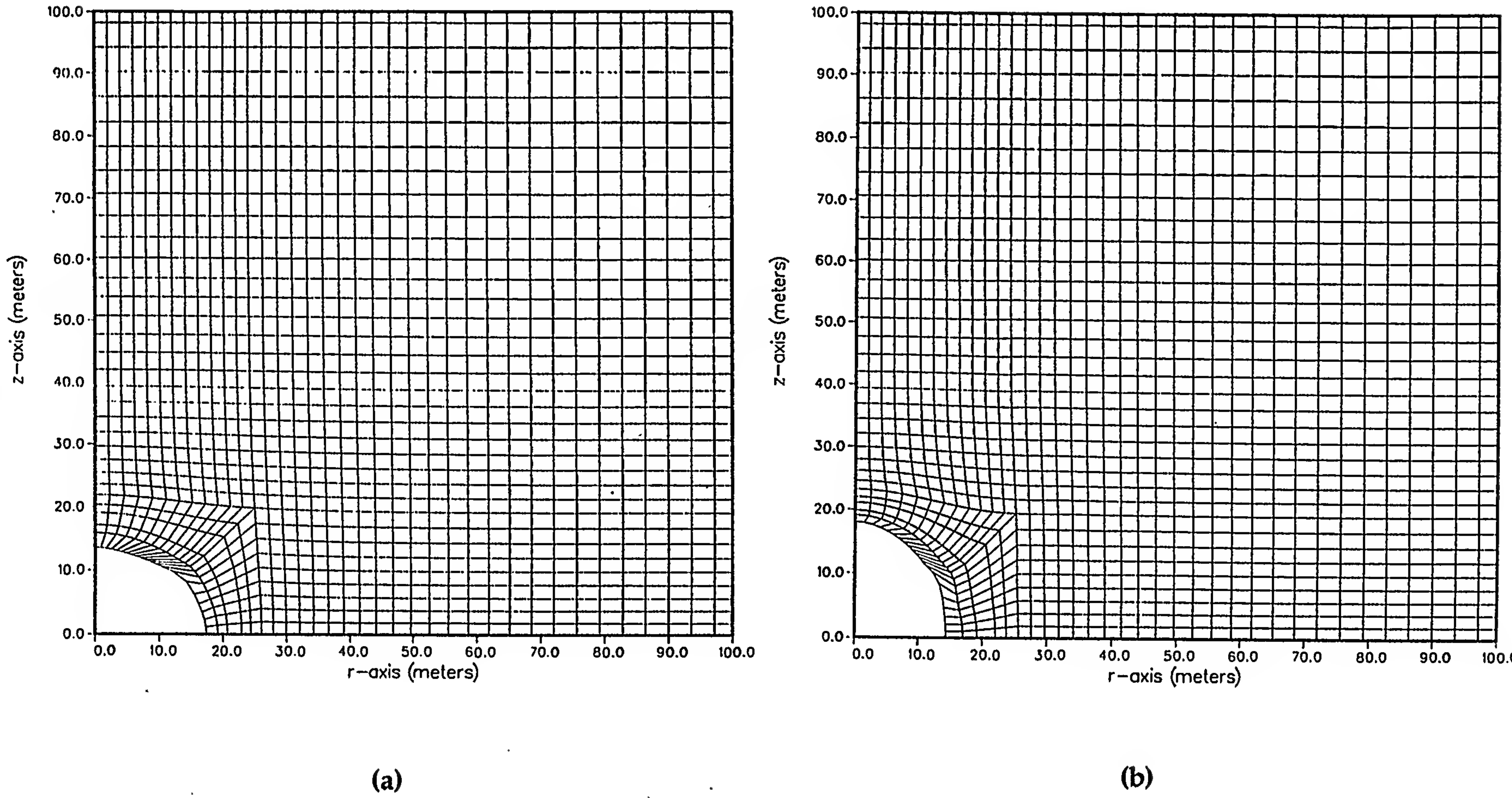


Figure 7. Cavity shapes for (a) Chem1 using the old tuff damage model, at 250 ms, and (b) Chem3 using the new damage model, at 350 ms.

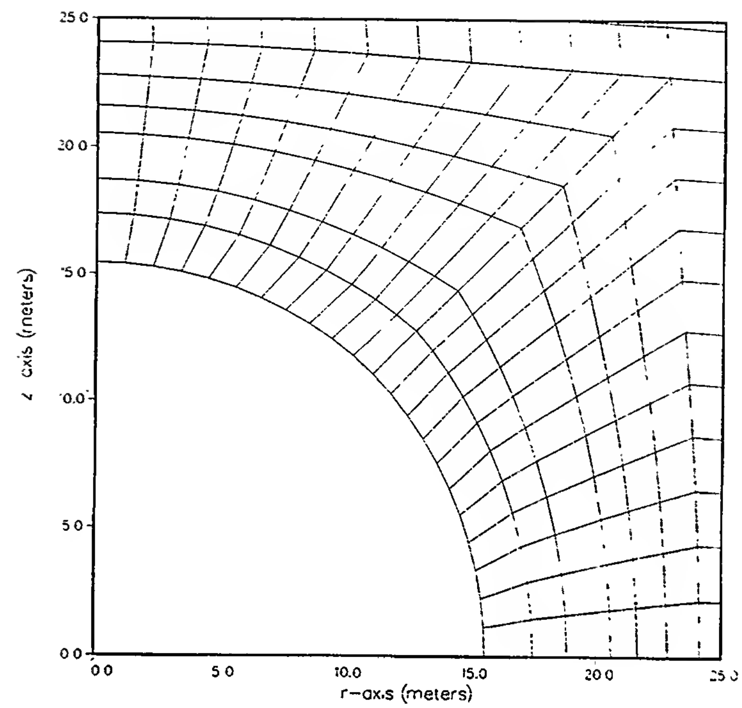


Figure 8. CRAM grid and cavity shape for Chem5 spherical HE source at a time of 350 ms.

The smaller cavity radii at all azimuths for Chem1 compared to Chem3 at rebound are consistent with the respective spherical source calculations, Runs 8a and 9a, and result from the differences in damaged strength used for the two tuff models. For the new damage model, the calculations also give a much larger core of very weak damaged tuff surrounding the cavity, which is responsible both for the very large average overshoots shown in Table 4, and for the larger temporal changes in cavity displacement which persist past 350 ms in Figure 9 (Cavity motions have virtually ceased in the calculations with the old damage model by 200 ms). At the average cavity rebound time of 108.6 ms for Chem3, cavity pressure has decreased to 54.5 bars, compared with 85.2 bars for Chem1 at its earlier rebound time. Since the maximum stress difference (strength) for the more severely damaged tuff of Chem3 at this cavity pressure is only 8.5 bars, (compared to the damaged strength of 68 bars for Chem1 at its rebound time) the tuff near the cavity has very little resistance to either outward or inward motions. The small cavity pressure cannot provide much added resistance to the anticipated cavity rebound.

At 108.6 ms, Chem3 gives an outward particle velocity of 33.4 m/sec at the cavity wall along the vertical axis of symmetry. Near the equator, the velocities at the cavity wall are an order of magnitude smaller, but are already directed inward. By 120 ms, these inward velocities have accelerated to 20 m/sec, roughly the same as the outward velocities above the cavity. Thus, the cavity rebound near the equator has a large head

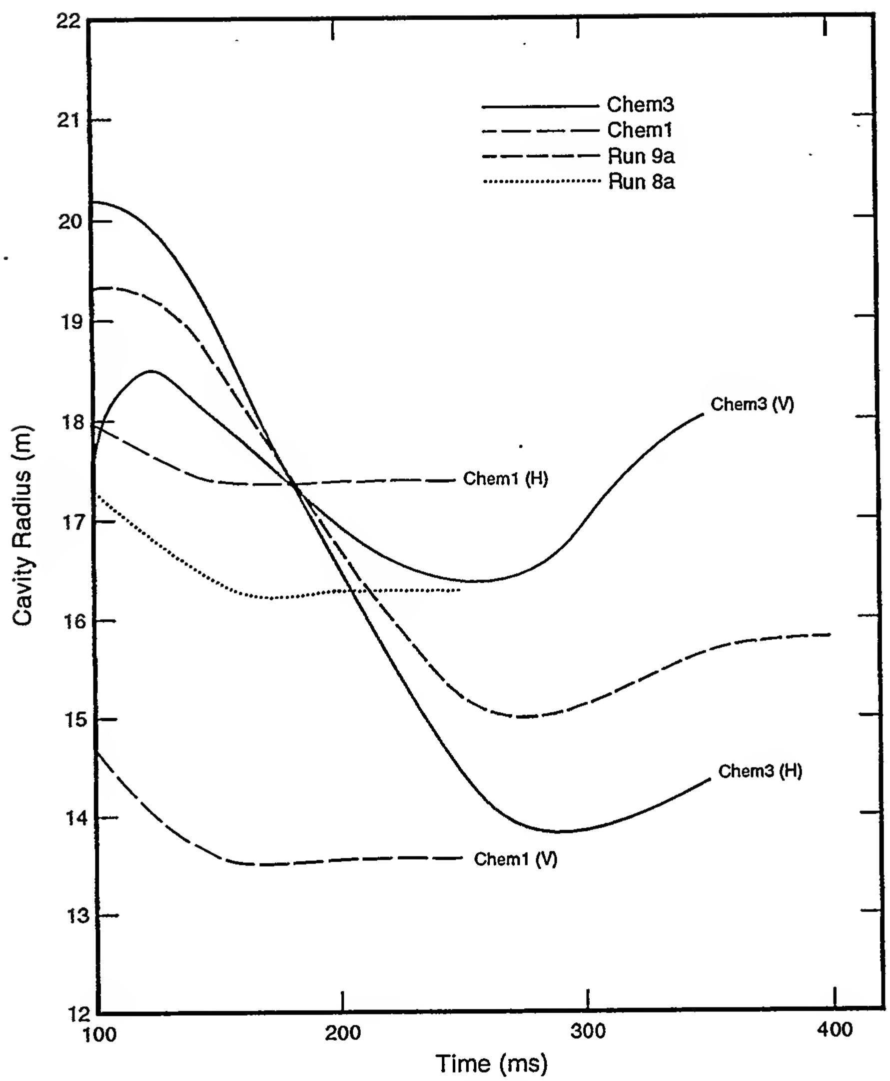


Figure 9. Cavity radii in vertical (V) and horizontal (H) directions *versus* time for Chem1, Chem3, and spherical Runs 8a and 9a.

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start on the rebound near the vertical axis of symmetry. Figure 10 shows the particle velocity vectors in the surrounding tuff at 150 ms for Chem3. (The tail of the vector indicates its location while its length shows its magnitude relative to the 50 m/sec arrow on top of the plot.) Velocities near the cavity are directed inward, but with a significant downward component. A maximum inward velocity of 43 m/sec is calculated for the cavity wall near the equator, a factor of three greater than seen directly above the cavity.

By 180 ms, pressure in the rebounding cavity has increased to 89.1 bars and the cavity is roughly spherical in shape. Inward velocities are at their largest, with the equatorial component, 51.1 m/sec, still a factor of three greater than above the cavity. Beyond this time, the cavity walls begin to decelerate as the cavity pressure increases

further. Due to the larger inward velocities along the equator, the cavity wall there coasts to a stop at a much later time. Thus, the major axis of the cavity becomes vertical. Note in Figure 9 that the spherical cavity rebound of Run 9a stops at an intermediate time and that the total cavity displacement during rebound is also intermediate between the horizontal and vertical cavity displacements of Chem3.

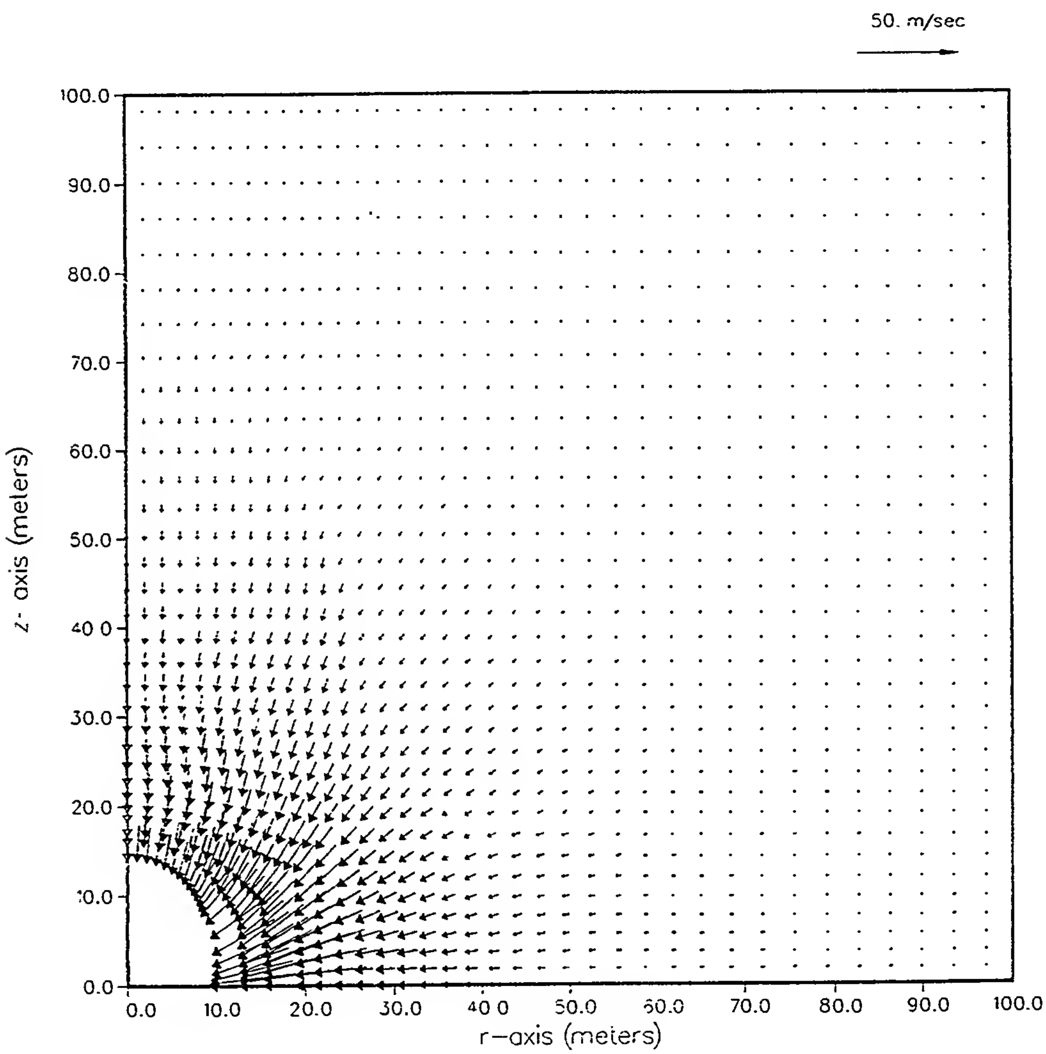


Figure 10. Particle velocity vectors at 150 ms for Chem3.

Maximum cavity pressure of 148.2 bars is reached at 275.2 ms for Chem3. Large cavity pressures at the end of rebound begin to push the very weak tuff surrounding the cavity outward once more. Since the rebound above the cavity has ended earlier, this part of the cavity wall now has a head start on the equatorial region and will be accelerated more. Thus, the outward recovery of the cavity after rebound is calculated to be largest along its vertical axis of symmetry, resulting in a still larger cavity eccentricity. Note in Figure 9 that, while the cavity recovery in the equatorial plane is a small fraction of the cavity wall displacement during rebound, the recovery above the cavity

is almost equal to the rebound displacement. Cavity recovery is still in progress at 350 ms when the calculation was terminated due to the imminent arrival of spurious signals from the outer boundaries of the SAGE grid. However, comparisons in Figure 9 with the results of spherical Run 9a indicate that cavity motions are nearly over.

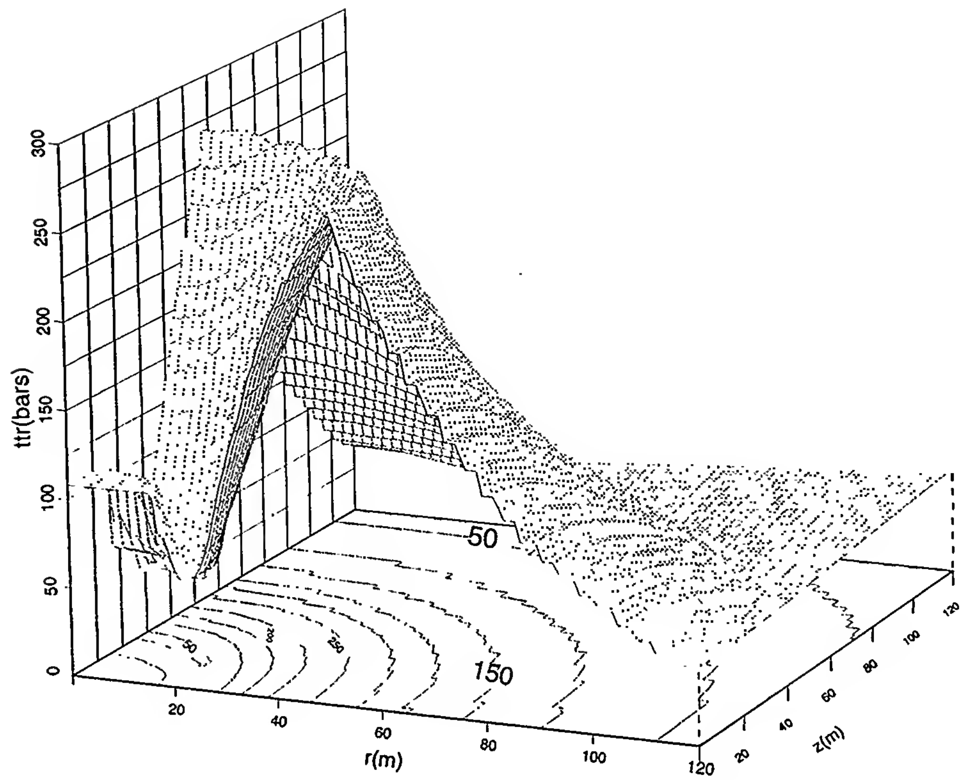
For Chem1, cavity rebound and recovery displacements in Figure 9 are quite small compared to the Chem3 results. Since the strength of the damaged tuff surrounding the cavity is an order of magnitude higher than calculated for Chem3 using the new damage model, these displacements are resisted primarily by the rock strength rather than by the cavity pressure. Thus, the relatively short duration rebound and recovery do not show even the same qualitative dependences on azimuth calculated for Chem3.

The residual stress fields calculated for Chem1 are very similar to those calculated using the old damage model in spherical Run 8a and Run 6, shown earlier as Figure 3. Although the radial range to the peak residual hoop stress, given in Table 4 in the equatorial plane for Chem1, is smaller than calculated for the spherical runs, the range to the hoop stress peak along the axis of symmetry for Chem1 is in good agreement with the spherical results. Figure 11 shows an isometric view of the in-plane residual hoop stress (ttr) calculated for Chem1 together with contours of this stress component. (Plots of the out-of-plane hoop stress are almost identical.) The cavity pressure of 108 bars, shown as a plateau at the left of the isometric, is much smaller than the surrounding hoop stresses at all azimuths outside of a 30-35 m radius. These large hoop stresses would be expected to prevent any hydrofracture which might be initiated at the cavity wall from propagating out to an open alcove located approximately 52 m from the cavity center.

For Chem3, the hoop stresses shown at 350 ms in Figure 12 (on the same scale as Fig. 11) have two distinct peaks as did the comparable Runs 7 and 9a, also made with the new tuff damage model. The peak nearest the cavity is roughly 35 bars above the cavity pressure at 350 ms for Chem3, but is only 23 bars above cavity pressure at 1.0 sec for the spherical runs. Beyond the first peak, hoop stresses are calculated to be substantially lower than cavity pressure out to well beyond the location of the open alcove before rising to the second peak. Thus, the possibility of HE detonation products reaching the open alcove may be interpreted as somewhat greater if the strength of the tuff conforms more to the new damage model, and will be investigated further.

The influence of the very different strength histories for Chem1 and Chem3 on the in-plane <u>principal</u> stresses surrounding the cavities can also be summarized using Figure 13. Lengths of the two lines drawn for each grid zone indicate the magnitudes of the principal <u>deviatoric</u> stresses, with the thicker line denoting the direction of the most compressive <u>total</u> stress. If needed, the magnitude of any given deviator can be determined by comparison with the length of the line shown above the plot which denotes the magnitude of the maximum deviator. However, these plots were developed primarily to show any changes in the orientation of the residual stress field, both with radial

(a)



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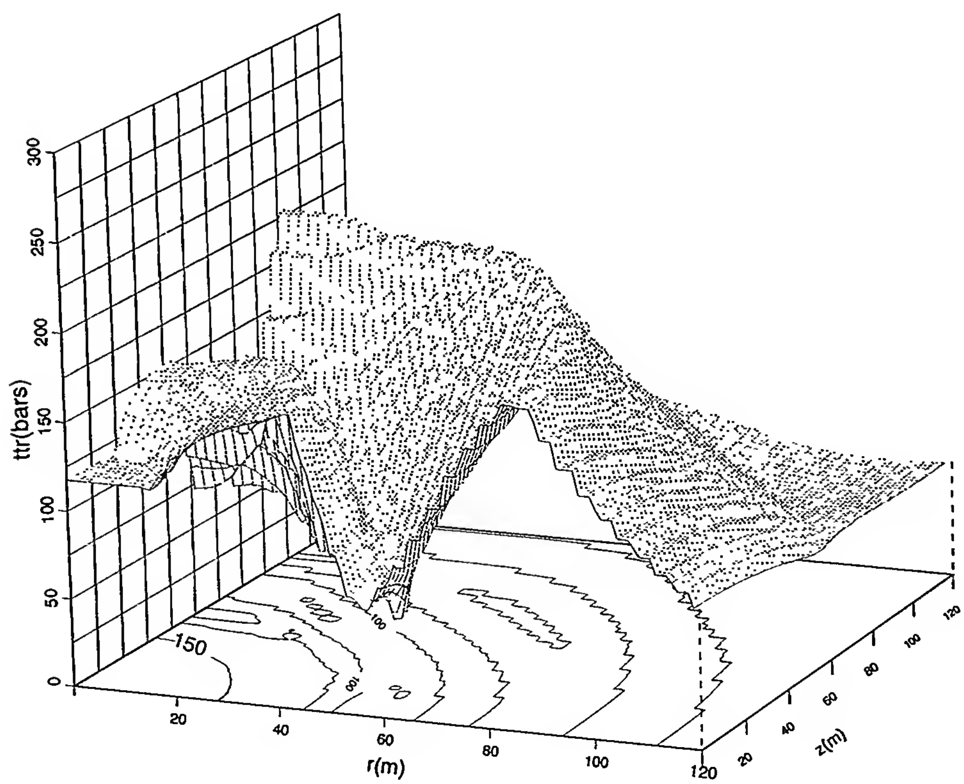
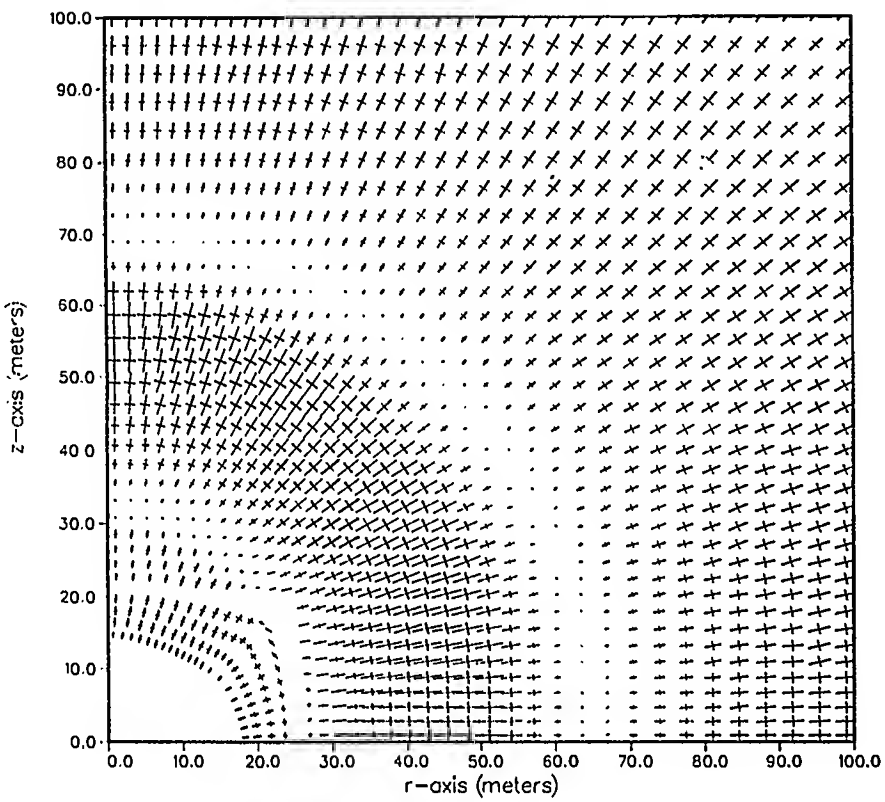
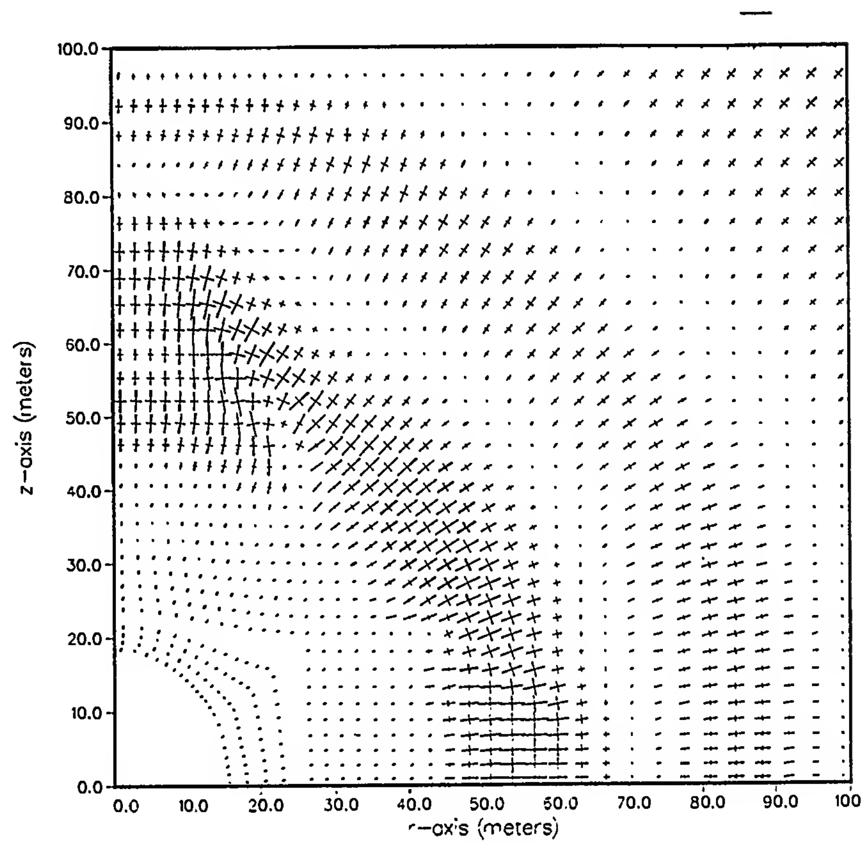


Figure 11. In-plane hoop stresses surrounding the (a) Chem1 cavity at 250 ms and (b) the Chem3 cavity at 350 ms, shown both as an isometric view and as a contour plot.



(a)



(b)

Figure 12. Magnitudes and directions of in-plane principal stress deviators at a) 250 ms for Chem1, and b) 350 ms for Chem3. A magnitude of deviatoric stress of 76.5 bars corresponds to the line shown above the plot.

range and with azimuth. The magnitudes of total stresses cannot be determined directly from these plots.

Immediately surrounding the Chem1 cavity, the most compressive residual total stress is more or less radial out to about 30 m. Deviators are small due to damage of the tuff. Beyond 30m and out to about 70 m, the most compressive stress is the hoop stress as expected. Crossover in the relative magnitudes of the two principal residual stress components is seen in Figure 12a as arcs of low deviatoric stress at radii of roughly 30 and 70 m. It should be noted that lines drawn along the near-radial stress directions from between 30 and 70 m intersect the z-axis well above the origin, while lines drawn from elsewhere intersect the z-axis below the origin.

Deviatoric stresses surrounding the Chem3 cavity at 350 ms are shown in Figure 12b to be very small due to the extreme damage out to a radius of approximately 45 m. This region ends with the first of the two hoop stress peaks. From 45 to 70+ m, the most compressive stresses are somewhat radial, corresponding to the near cavity region for Chem1. However, the orientations are more random than for Chem1 in this region, perhaps because the cavity has not yet come to rest. Between 80 and 100 m, hoop stresses are a maximum with the near radial stress lines intersecting the z-axis above the origin. This region contains the second residual stress peak which, due to the large extent of very weak rock around the cavity for Chem3, is radially much further out than the single peak calculated for Chem1.

Containment is not really at issue here in that any gas escaping into the open tunnel complex from this HE cavity can eventually be removed. However, the containment implications of the very different residual stress fields calculated in Chem1 and Chem3 are of considerable interest. These were analyzed further in several two-dimensional calculations made using the hydrofracture algorithms in the F-Cubed code. These calculations, Chem2, with the old damage model, and Chem4, with the new damage model, both assumed a single, worst case hydrofracture propagating through the tuff along the equatorial plane of symmetry, but were otherwise set up to be identical to Chem1 and Chem3 respectively. Both Chem2 and Chem4 were repeated using several different hydrofracture options. In addition, several calculations were made with the FAST hydrofracture code (Ref. 13), using hoop stresses *versus* time obtained from spherical Run 9a, for comparison with the F-Cubed calculations. FAST contains the same modeling for the fluid in the fracture as F-Cubed, but only a linear elastic model for the surrounding tuff. The intent of these calculations was to evaluate the possibility of HE detonation products flowing from the cavity to the open alcove 52 m away (and from there through the open tunnel complex) along either a pre-existing plane of weakness or an event produced hydrofracture.

The maximum hydrofracture lengths obtained from the seven calculations are summarized in Table 5. (Hydrofracture lengths are measured from the wall of the cavity.) Both calculations made using the old damage model show a worst case hydrofracture stopping at roughly 30-32 m from the center of the HE charge, *i.e.*, at the range at which residual hoop stresses were calculated to increase dramatically. Calculation

Chem2, using more realistic modeling assumptions, including fluid friction and heat transfer to the walls of the fracture, gave a slightly shorter fracture than obtained for Chem2f, which assumed that the fluid pressure everywhere along the open fracture was equal to cavity pressure. Note that a number of shorter fractures is more likely than the single worst case fracture postulated here.

Table 5. Calculated hydrofracture lengths.

Run #	Chem2	Chem2f	Chem4	Chem4f	Chem4f2	Fast1[a]	Fast2[a]
Damage Model	old	old	new	new	new	G=35[b] new	G=10 new
Fracture Model	fr,ht[c]	pcav[d]	fr,ht	pcav	open[e], pcav	fr,ht	fr,ht
Fracture Length	12.6	14.8	2.0	2.0	7.8	5.6	14.6

a) FAST code runs used hoop stresses vs time from spherical Run 9a.
b) Shear modulus in kb.
c) Fluid friction and heat transfer to walls.
d) Cavity pressure applied at fracture tip.
e) Fracture aperature forced to remain open with 2.0 pct crack strain.

Both Chem4 and Chem4f, made using the new damage model, indicated that a hydrofracture could not propagate through the much weaker damaged tuff calculated to surround the cavity. The 2.0 m lengths shown in Table 5 are the initial fracture lengths used by the model to initiate the fracture propagation. By the end of the calculations, the fracture is fully closed. In an attempt to artificially prop open the fracture, Chem4f2 forced the fracture aperture (at the cavity wall) to remain open with a 2.0 pct crack strain even when the surrounding stresses tended to force this aperture to close. Even with this assumption, the fracture propagated less than 8.0 m from the cavity wall. The FAST code using the low stress fields calculated in Run 9a, propagated a slightly longer fracture, only when a very small shear modulus was assumed. A small shear modulus near the cavity is compatible with post-shot observations of low shear wave velocities near the chimney produced by a nuclear event.

It should be noted that an event produced hydrofracture has never been observed for a <u>tamped</u> nuclear event in the zeolitized tunnel tuff beneath Rainier Mesa, although for the partially decoupled RED HOT cavity event, significant hydrofractures from the cavity were observed on mineback. Occasionally, seams of altered tuff containing radioactivity have been encountered during tunnel mining operations. These observations have usually been attributed to seepage of cavity gas along pre-existing planes of weakness, such as interfaces between bedding planes, from earlier tamped nuclear events located as far as 2-3 cavity radii away.

Minebacks after a number of HE events conducted by C. Smith of SNL (using TNT charges of 8, 64, 1000, and 2000 lb) in similar tuffs also showed no hydrofractures

from tamped cavities, although the more decoupled Junior Jade cavity events (Ref. 15) showed a number of hydrofractures. (A tamped TNT event, RS 18, in a welded tuff bed having much higher shear strength, also showed hydrofractures.) Proffer, *et al* (Ref. 16) has showed that the hydrofracture lengths calculated using the F-Cubed code are consistent with those observed for Junior Jade and that hydrofractures are not calculated to propagate for the comparable tamped or very lightly decoupled HE events. The computer results given in Table 5 also suggest that a hydrofracture is unlikely to propagate easily through a "very weak" medium even when the calculated residual stress fields appear somewhat threatening. Thus, the melt surrounding a tamped nuclear event or the very weak "onion skin layers" of tuff observed on mineback after the tamped HE events may retard or prevent hydrofracture even if the tuff further from the cavity is stronger than given by the calculations with the new damage model described here.

Although the cavity pressures calculated here at the end of dynamic cavity growth and rebound are very similar for nuclear and HE sources, the HE detonation products are calculated to contain 50 percent (by weight) non-condensable gases at roughly a factor of three lower average temperature, 540 degrees Kelvin. Recent calculations by Lie and Peterson (Ref. 17) showed, for the large mass of lower temperature CHEMICAL KILOTON cavity gas, the subsequent pressure decay due to cooling and seepage through the cavity walls to be very slow compared to a nuclear event. They calculated a pressure of about 30 bars and a temperature of about 350 degrees Kelvin for the HE cavity gases 100 days after detonation, assuming that the cavity does not collapse before then. Thus, unless cavity collapse occurs much earlier, which is likely, cavity gas would be expected to seep through the stemming grouts in the emplacement drift and into the open alcove even if hydrofracture does not occur.

5. CONCLUSIONS

The spherically symmetric ground motion calculations for HE and nuclear sources described in this paper lead to the following conclusions:

- Containment related results for HE and nuclear sources, such as residual stress magnitudes and locations, and ground motion pulse shapes, are qualitatively very similar in spite of the large differences in initial source dimensions and energy densities.

- For the HE sources, these results are not very sensitive to variations in the JWL equation of state coefficients used to model the detonation products.

- For both the HE and nuclear sources, the containment related results are much more sensitive to the details of the constitutive models used to describe the tuff damage behavior.

- Based on comparisons between calculated cavity radii, rebound times, yield radii, and RDP's, the CHEMICAL KILOTON HE source couples about 50 pct stronger

than a nuclear source in the same tuff. (The 1 kt HE source looks very much like a 1.5 kt nuclear source.)

- Based on comparisons between calculated peak stresses, the HE source couples 64-85 pct stronger, depending upon stress level.

- Based on comparisons between calculated peak displacements, the HE source couples 90-120 pct stronger, with the higher coupling for the new damage model.

- The more efficient coupling of the HE source can be attributed to the high gamma (1.3) of its expanded detonation products, compared with the relatively low gamma of the saturated tuff gas in the expanded nuclear cavity.

Use of empirical fits to ground motion data from nuclear events and one HE event (ONETON) in tuff and the spherically symmetric ground motion calculations of this study lead to the following conclusions:

- The tuff site of interest here should couple higher in peak stress for either a nuclear or HE event than the average tuff site which contains 1-2 pct gas porosity due to its relatively low gas porosity (0.5 pct).

- Calculations with the variable gamma cavity equation of state for tuff are much more consistent with the empirical peak stress data from nuclear events in tuff than those made using a constant gamma of 1.3.

- Calculations with the new damage model are in better agreement with peak displacement data from nuclear events than those made with the old model.

- The ONETON HE event couples 80-100 pct stronger than a nuclear event based on empirical fits to peak stress data. The limited peak displacement data for this HE event also shows enhanced coupling.

In summary, the ground motion calculations and empirical data show that an HE event in tuff should couple from 50 to 100 pct stronger than a nuclear event in the same tuff.

The axisymmetric two-dimensional ground motion calculations for the HE source lead to the following conclusions:

- A dynamic hydrofracture from the HE cavity out to the open alcove at a range of roughly 52 m is extremely unlikely. Calculations made using the old tuff damage model show that a single worst case fracture cannot propagate through the large compressive residual stresses surrounding the cavity.

- Calculations with the new damage model show that a hydrofracture will not propagate through very weak, damaged tuff which cannot support compressive hoop

stresses significantly larger than cavity pressure. Thus, the melt and/or damaged tuff surrounding a tamped nuclear event or the very weak "onion skin layers" of tuff observed on mineback after tamped HE events may provide an explanation for the absence of observed dynamic hydrofractures on such tamped events in tuff.

- The large mass of non-condensable HE cavity gas is calculated to be at roughly the same pressure at the end of dynamic cavity growth but a factor of three lower average temperature than for a nuclear cavity. Thus, cavity pressure decay is calculated to be very slow (A pressure of about 30 bars remains after 100 days unless the cavity collapses.), so that seepage of cavity gas through the stemming grouts in the emplacement drift and into the open alcove would be expected.

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